PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXV. For the Year 1785.

PART II.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXV.

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

CONTENTS

OF

VOL. LXXV. PART II.

XVI. *Of the Rotatory Motion of a Body of any Form whatever, revolving, without Reſtraint, about any Axis paſſing through its center of Gravity.* By *Mr.* John Landen, *F.R.S.* page 311

XVII. *Deſcription of a new Marine Animal. In a Letter from Mr.* Everard Home, *Surgeon, to* John Hunter, *Eſq. F.R.S. With a Poſtſcript by Mr.* Hunter, *containing anatomical Remarks upon the ſame.* p. 333

XVIII. *A Deſcription of a new Syſtem of Wires in the Focus of a Teleſcope, for obſerving the comparative Right Aſcenſions and Declinations of cœleſtial Objects; together with a Method of inveſtigating the ſame when obſerved by the Rhombus, though it happen not to be truly in an equatorial Poſition.* By the *Rev.* Francis Wollaſton, *LL.B. F.R.S.* p. 346

XIX. *An Account of a Stag's Head and Horns, found at* Alport, *in the Pariſh of* Youlgreave, *in the County of* Derby. *In a*

Letter from the Rev. Robert Barker, *B.D. to* John Jebb, *M.D. F.R.S.* p. 353

XX. *An Account of the ſenſitive Quality of the Tree* Averrhoa Carambola. *In a Letter from* Robert Bruce, *M.D. to Sir* Joſeph Banks, *Bart. P.R.S.* p. 356

XXI. *An Account of ſome Experiments on the Loſs of Weight in Bodies on being melted or heated. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joſeph Banks, *Bart. P.R.S.* p. 361

XXII. *Sketches and Deſcriptions of three ſimple Inſtruments for drawing Architecture and Machinery in Perſpective. By Mr.* James Peacock; *communicated by* Robert Mylne, *Eſq. F.R.S.* p. 366

XXIII. *Experiments on Air. By* Henry Cavendiſh, *Eſq. F.R.S. and A.S.* p. 372

XXIV. *An Account of the Meaſurement of a Baſe on* Hounſlow-Heath. *By Major-General* William Roy, *F.R.S. and A.S.* p. 385

XXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, in Rutland, 1784. *By* Thomas Barker, *Eſq. Alſo of the Rain at* South Lambeth, Surrey; *and at* Selbourn *and* Fyfield, Hampſhire. *Communicated by* Thomas White, Eſq. *F.R.S.* p. 481

PHILOSOPHICAL TRANSACTIONS.

XVI. *Of the Rotatory Motion of a Body of any Form whatever, revolving, without Reſtraint, about any Axis paſſing through its center of Gravity.* By *Mr.* John Landen, *F.R.S.*

Read March 17, 1785.

A SPHERICAL body, uniformly denſe, it is obvious, will, if made to revolve freely about any axis paſſing through its center, continue to revolve about the ſame axis; and, by what I have ſhewn in the *Philoſophical Tranſactions* for the year 1777, it appears, that a *cylinder* of uniform denſity, whoſe length is to its radius as $\sqrt{3}$ to 1, will do the ſame. It likewiſe appears, by my *Mathematical Memoirs*,

that a *cone*, a *conoid*, a *priſm*, or a *pyramid*, &c. of certain dimenſions, will have the like property of continuing, without any reſtraint, to revolve about any axis paſſing through its center of gravity.

When the axis, about which a body may be made to revolve, is not a permanent one, the centrifugal force of its particles will diſturb its rotatory motion, ſo as to cauſe it to change its axis of rotation (and conſequently its poles) every inſtant, and endeavour to revolve about a new one: and I cannot think it will be deemed an unintereſting propoſition to determine in what track, and at what rate, the poles of ſuch momentary axis will be varied in any body whatever; as, without the knowledge to be obtained from the ſolution of ſuch problem, we cannot be certain whether the earth, or any other planet, may not, from the *inertia* of its own particles, ſo change its momentary axis, that the poles thereof ſhall approach nearer and nearer to the preſent equator, or whether the evagation of the momentary poles, ariſing from that cauſe, will not be limited by ſome known leſſer circle. Which certainly is an important conſideration in aſtronomy; eſpecially now that branch of ſcience is carried to great perfection, and the acute aſtronomer endeavours to determine the motions of the heavenly bodies with the greateſt exactneſs poſſible.

I do not know that the problem has before been ſolved by any mathematician in theſe kingdoms; but I am aware that it has been conſidered by ſome gentlemen, very eminent for their mathematical knowledge, in other nations. The ſolutions of it, given by the celebrated M. LEONHARD EULER and M. D'ALEMBERT, I have ſeen: and we learn from what the laſt mentioned gentleman has ſaid, in his *Opuſcules Mathematiques*, that

that a ſolution of it, inveſtigated by M. JOHN ALBERT EULER (after a method ſimilar to his father's) obtained the prize given by the Academy of Sciences in the year 1761. The concluſions deduced by thoſe very learned gentlemen differing greatly from mine made me ſuſpect, for ſome time, that I had ſomewhere erred in my inveſtigation, and induced me to reviſe my proceſs again and again with the greateſt circumſpection. At length my ſcrutiny has ſo removed my doubts, that, being well aſſured of the truth of my theory, I now beg leave to preſent it to the Royal Society; preſuming that it will be found not unworthy of the notice of ſuch readers, as are curious in contemplating the various motions which bodies may naturally have, in conſequence of inſtantaneous or continued impulſe.

In the *Philoſophical Tranſactions* referred to above, I gave a ſpecimen of this theory, as far as it relates to the motion of *a ſpheroid* and *a cylinder*. The improvements I have ſince made in it, enable me now to extend it to the motion of *any body whatever*, how irregular ſoever its form may be.

What I here infer therefrom will be found to differ very materially from the deductions in the ſolutions given by the gentlemen above-mentioned. They repreſent the angular velocity, and the momentum of rotation of the revolving body, as always *variable*, when the axis about which it has a tendency to revolve is a momentary one, except in a particular caſe. By my inveſtigation it appears, that the angular velocity and the momentum of rotation will always be *invariable* in any revolving body, though the axis about which it endeavours to revolve be continually varied; and the tracks of the varying poles upon the ſurface of the body are thereby determined with great facility.

It is not only obſervable, that the tracks which the varying poles take, in the ſurface of any revolving body, are ſuch that its momentum of rotation may continue the ſame whilſt its angular velocity continues the ſame; but it may be obſerved, that, in any given body, there is only one ſuch track which a momentary pole can purſue from any given point.

If the angular velocity and the momentum of rotation of a revolving body were to vary according to the computations adverted to above, it would follow, that a body might acquire an increaſe of force from its own motion, without being any way affected by any other body whatever, as the ſame percuſſive force, applied at the ſame diſtance from the momentary axis, would not always deſtroy the rotatory motion of the body, which ſurely cannot poſſibly be true. From the principles or laws of motion, which I conſider as undoubtedly true (and which indeed are no other than the common principles of mechanics), I conclude that a revolving body, not affected by any external impulſe, can no more acquire an increaſe in its momentum of rotation, than any other body, moving freely, can acquire an increaſe in its momentum, or quantity of motion, in any given direction, without being impelled by gravity or ſome other force. And the truth of this concluſion (which is hereinafter proved by other reaſoning) may be eaſily inferred from the property of the lever; ſeeing that the joint centrifugal force of the particles of the revolving body (which is the *only* diſturbing force) has no tendency to accelerate or retard their motion about the momentary axis, but only to alter the poſition of ſuch axis, the direction in which that force acts being always in a plane wherein that axis will be found.

By the theory explained in this paper, it appears that a *parallelopipedon* may always be conceived of ſuch dimenſions,

that being, by ſome force or forces, made to revolve about an axis, paſſing through its center of gravity, with a certain angular velocity, it ſhall move exactly in the ſame manner as any other given body will move, if made to revolve, by the ſame force or forces, about an axis paſſing through its center of gravity; the quantity of matter (as well as the initial angular velocity) being ſuppoſed the ſame in both bodies; and due regard being had, in the application of the moving force or forces, to the correſponding planes in the bodies. Therefore, as we may from thence always aſſign the dimenſions of a *parallelopipedon* that ſhall be affected exactly in the ſame manner as any other given body will be affected, as well with regard to the centrifugal force of the reſpective particles of the bodies, as to the action of equal percuſſive forces, or oſcillation; it will, after ſhewing how the dimenſions of ſuch *parallelopipedon* may be computed, be only neceſſary, in inveſtigating the propoſition under conſideration, to determine the tracks and velocities of the poles of the momentary axis, about which any *parallelopipedon* may be made to revolve.

Firſt then to find ſuch parallelopipedon (P), that, with reſpect to the action of ſuch forces as are mentioned above, it may be affected exactly in the ſame manner as any other given body (Q). Let it be conſidered that G (tab. X. fig. 1.) being the center of gravity, N a point of ſuſpenſion, and O the correſponding center of oſcillation or percuſſion, the rectangle GN × GO will be an invariable quantity, the direction NGO continuing the ſame; and that a cylindric ſurface being deſcribed, ſuch that the center of the middle circular ſection thereof ſhall be G, and radius $= \sqrt{GN \times GO}$, and whoſe axis ſhall be perpendicular to the plane wherein the line NGO is ſuppoſed to be impelled to move; if all the matter in the body were placed

any

any where in that ſurface, ſo that G ſhould be the center of gravity of the matter ſo placed, any given force or forces, acting on the body in the plane juſt now mentioned, would cauſe the line NGO in the body to move exactly in the ſame manner as it would move, if it were carried with the matter placed in the ſaid ſurface (as before-mentioned) after having been put in motion by the action of the ſame force or forces. Moreover, let it be conſidered, that there will at leaſt be three permanent axes of rotation in the body Q, at right angles to each other (as I have proved in my *Mathematical Memoirs*); and that, ſuppoſing NGO to coincide with thoſe three axes in three ſucceſſive caſes wherein the matter in Q ſhall, in each caſe, be conceived to be placed in a cylindric ſurface as deſcribed above, we may conceive it poſſible ſo to place the matter of the body, that all of it ſhall be in each of thoſe three ſurfaces, and G ſtill continue its center of gravity. And, a computation being made accordingly, it appears, that the matter of the body Q muſt be placed, in equal quantities, at each of the eight angular points of a *parallelopipedon* (R) whoſe dimenſions (length, breadth, and thickneſs) ſhall be $\sqrt{2d^2+2f^2-2e^2}$, $\sqrt{2e^2+2f^2-2d^2}$, and $\sqrt{2d^2+2e^2-2f^2}$; d, e, and f, being the three values of $\sqrt{GN \times GO}$, when NGO is ſucceſſively a permanent axis of rotation, with reſpect to the body Q, in three directions at right angles to each other.

If Q were a parallelopipedon, it may be eaſily proved, that its dimenſions muſt be $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6d^2}$, and $\sqrt{6d^2+6e^2-6f^2}$, that the correſponding parallelopipedon, at the angular points whereof the matter of Q is conceived to be placed as above, may have the ſame dimenſions as thoſe which we have found our parallelopipedon R muſt have.

Whence we may infer, that the parallelopipedon ($P^{\prime}$, which we propoſed to find, muſt have the dimenſions laſt written; namely, length, breadth, and thickneſs, reſpectively equal to $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6d^2}$, and $\sqrt{6d^2+6e^2-6f^2}$: which may be confirmed by a more ſtrict demonſtration founded on the principles made uſe of in my *fourth Memoir*. For it appears by what is there proved, that the centrifugal forces of the particles of any revolving body, in two directions at right angles to each other, may be expreſſed in terms of A, B, K, and variable quantities ſhewing the poſition of the momentary axis; and that, in a parallelopipedon whoſe dimenſions (length, breadth, and thickneſs) are a, b, k; and whoſe maſs, or content, is $=M$; A will be $=\frac{Ma^2}{12}$, $B=\frac{Mb^2}{12}$, and $K=\frac{Mk^2}{12}$. If therefore a be $=\sqrt{6d^2+6f^2-6e^2}$, $b=\sqrt{6e^2+6f^2-6d^2}$, and $k=\sqrt{6d^2+6e^2-6f^2}$; in ſuch body,

$$\text{A will be} = \frac{M}{2}\times\overline{d^2+f^2-e^2},$$

$$\text{B} = \frac{M}{2}\times\overline{e^2+f^2-d^2},$$

$$\text{K} = \frac{M}{2}\times\overline{d^2+e^2-f^2}.$$

But, in any body whatever,

$M\times d^2$ is $=$ the ſum of all the $\overline{x^2+z^2}\times p$,

$M\times e^2$ $=$ the ſum of all the $\overline{y^2+z^2}\times p$,

$M\times f^2$ $=$ the ſum of all the $\overline{x^2+y^2}\times p$,

and $\frac{M}{2}\times\overline{d^2+e^2+f^2}=$ the ſum of all the $\overline{x^2+y^2+z^2}\times p$: x, y, and z correſponding to the place of the particle p in the body; x being meaſured from the center of gravity upon a permanent axis of rotation, y at right angles to x, and z at right angles

to

to y in a plane to which the ſaid axis is perpendicular. Therefore,

A, which is = the ſum of all the $x^2 \times p$, will be $= \frac{M}{2} \times \overline{d^2 + f^2 - e^2}$,

B, = the ſum of all the $y^2 \times p$, $= \frac{M}{2} \times \overline{e^2 + f^2 - d^2}$,

K, = the ſum of all the $z^2 \times p$, $= \frac{M}{2} \times \overline{d^2 + e^2 - f^2}$,

Hence it is evident, that d, e, and f being determined from any body whatever, the values of A, B, and K will be the ſame in that body as in our parallelopipedon P; and that the centrifugal forces of the particles will be the ſame in both bodies. Conſequently, their motions about ſucceſſive momentary axes (whoſe poles are varied by the perturbation ariſing from thoſe forces), will be the ſame in both bodies; their initial angular velocities being the ſame; as well as the poſition of their initial momentary axes, with reſpect to the correſpondent permanent axes of rotation in each body.

Let us now proceed to find how any *parallelopipedon* will revolve about ſucceſſive momentary axes paſſing through its center of gravity: by which means, with the help of the theorem juſt now inveſtigated, we ſhall be enabled to define how any body whatever will revolve about ſuch axes; which is the chief purpoſe of this diſquiſition.

Fig. 2. and 3. The length, breadth, and thickneſs of the revolving *parallelopipedon* (P) being $2d$, $2c$, and $2b$, conceive a ſpherical ſurface without matter, whoſe center is the center of gravity of the body P, to be carried about with that body during its motion; and let the ſaid ſurface be orthographically projected, ſo that the radius upon which b is meaſured may be repreſented

repreſented by AB; the radius upon which d is meaſured may be repreſented by AD; and the radius AC, upon which c is meaſured, may be projected into the central point A. Let P be the momentary pole, and PQ the continuation of the great circle CP. Let a denote the radius AB (= AD); g and γ the ſine and coſine of the arc CP; s and t the ſine and coſine of the arc BQ, to the ſame radius a; e the angular velocity of the body and ſpherical ſurface, meaſured at the diſtance a from the momentary axis; and M the maſs or content of the parallelopipedon ($= 8bcd$).

Then the motive force E, urging the pole P towards Q, will (by what I have proved in my *Mathematical Memoirs*) be $= \frac{Me^2g\gamma}{3a^7} \times \overline{Ds^2 - Ca^2}$; and the motive force $\ddot{E}$, urging the ſame pole in a direction Po, at right angles to that in which E acts, $= \frac{Me^2g}{3a^6} \times Dst$; C and D being equal to $c^2 - b^2$ and $d^2 - b^2$ reſpectively. Let Pq be to Po as E to $\ddot{E}$; complete the parallelogram oPqr, and draw the diagonal Pr. This laſt mentioned line will (by what I have ſhewn in the *Philoſophical Tranſactions* for the year 1777) be perpendicular to the tangent to the polar track at P. Therefore P$\ddot{p}\dddot{p}$ being the projection of that track, and Pp an indefinitely ſmall particle thereof; if pu be perpendicular to PuA, and the quantities $d^2 - c^2$, $c^2 - b^2$, be not negative; $\frac{\gamma}{a} \times \overline{Ds^2 - Ca^2}$ will be to Dst (as Pq to Po) as pu to Pu, the triangles Por and Pup being ſimilar, and $or = Pq$. But with reſpect to our ſpherical ſurface, pu will be to Pu as $\frac{\dot{g}s}{t}$ to $-\frac{a\dot{g}}{\gamma}$; therefore, $\overline{Ca^2 - Ds^2} \times \dot{g}$ will be $= Dgss\dot{}$, and $\frac{\dot{g}}{g} = \frac{Dss\dot{}}{Ca^2 - Ds^2}$. Whence, by taking the

fluents, we have $s^2 = a^2 \times \frac{Dm^2 - C\gamma^2}{Dg^2}$, and $t^2 = a^2 \times \frac{Dn^2 - B\gamma^2}{Dg^2}$; m and n denoting the values of s and t, when g is $= a$ and $\gamma = 0$; and B being put to denote the difference $D - C = d^2 - c^2$.

If now β and δ be put to denote the coſines of BP and DP to the radius a, we ſhall, from what is done above, have

$$\beta = \frac{gt}{a} = \frac{\sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}}},\quad \delta = \frac{gs}{a} = \frac{\sqrt{Dm^2 - C\gamma^2}}{D^{\frac{1}{2}}};$$

$$\beta^2 + \gamma^2 + \delta^2 = a^2,\quad \beta\dot{\beta} + \gamma\dot{\gamma} + \delta\dot{\delta} = 0;$$

$$b^2\beta^2 + c^2\gamma^2 + d^2\delta^2 = b^2n^2 + d^2m^2,\text{ and } b^2\beta\dot{\beta} + c^2\gamma\dot{\gamma} + d^2\delta\dot{\delta} = 0.$$

Drawing AR ſo that $D^{\frac{1}{2}} \times$ fine of BR ſhall be $= C^{\frac{1}{2}}a$, it is *very remarkable*, that the momentary pole (P) will run round about the point B, or about the point D, in the ſpherical ſurface, according as the initial pole ſhall be in the part BCR or DCR of the ſaid ſurface; that is, according as Dm^2 is leſs or greater than Ca^2: and that, if the initial pole (P) be any where in the great circle CR, the momentary pole, keeping in the arc of that circle, will continually approach nearer and nearer to the point C in the ſurface of the ſphere; but, by what follows, we ſhall find that it never can arrive at that point in any finite time!

The equation of the track of the pole in the projection to which we have hitherto referred will, it is now obvious, be $y^2 = \frac{B}{C} \times x^2 + \frac{Ca^2 - Dm^2}{B}$; x, meaſured from the center A upon AD, being $= \delta$; and y, at right angles thereto, $= \beta$.

If C be $= 0$ (that is, if c be $= b$), x will be equal to the invariable quantity m; the projected track, a *right line* parallel to AB; and the track upon the ſurface of the ſphere, a *leſſer circle* in a plane parallel to the plane of the great circle BC.

If C be $=D$, y will be equal to the invariable quantity n; the projected track, a *right line* parallel to AD; and the track on the ſurface of the ſphere, a *leſſer circle* in a plane parallel to the plane of the great circle CD.

If Dm^2 be $=Ca^2$ the projected track will be the *right line* AR, and $y=\overline{\frac{B}{C}}^{\frac{1}{2}} \times x$; the track upon the ſurface of the ſphere being the *great circle* CR.

In all other caſes in this projection, the track will be an *hyperbola* whoſe center is A, ſemi-axis $Aa=\overline{\frac{Ca^2 \sim Dm^2}{B}}^{\frac{1}{2}}$, and the other ſemi-axis $=\overline{\frac{Ca^2 \sim Dm^2}{C}}^{\frac{1}{2}}$; the right line AR being always an *aſypmtote*.

Fig. 4. When the track is projected on a plane ACD, to which the radius AB is perpendicular (the point D being the vertex as before) the equation thereof will be $y^2=\frac{D}{C} \times \overline{m^2-x^2}$; x, meaſured from the center A upon AD, being $=\delta$ (as before); and y, at right angles thereto $=\gamma$. This projection of the track of the pole will therefore always be an *ellipſis* a b (or a *circle*) whoſe center is A; ſemi-axis A a $=m$; and the other ſemi-axis $=\overline{\frac{D}{C}}^{\frac{1}{2}} \times m$: except c be $=b$; in which caſe the projected track will be a *right line* a b parallel to AC.

Fig. 5. Moreover, the equation of the track projected on the plane ABC, to which the radius AD is perpendicular, will be $y^2=\frac{D}{B} \times \overline{n^2-x^2}$; x, meaſured from the center A upon AB, being $=\beta$; and y, at right angles thereto, $=\gamma$. The track of the pole in this projection will therefore always be an *ellipſis*

a b (or a *circle)* whoſe center is A; ſemi-axis A a = n; and the other ſemi-axis = $\left|\frac{D}{B}\right|^{\frac{1}{2}} \times n$; except c be = d; in which caſe the projected track will be a *right line* a b parallel to AC.

With regard to the permanent axes of rotation of our parallelopipedon, it appears, by my *Mathematical Memoirs*, that if two of its dimenſions be equal (that is, when the body is a *ſquare priſm)*, any line paſſing through the center of gravity of the body, in a plane to which the other dimenſion is perpendicular, will be a permanent axis of rotation; as will the line paſſing through that center, at right angles to that plane. If all the three dimenſions be equal (that is, when the body is a *cube)*, any line whatever paſſing through the center of gravity of the body will be a permanent axis of rotation.

It is obſervable, that the momentum of rotation of the body, about the momentary axis, is found by computation always = $\frac{e}{a^2} \times \sqrt{b^2m^2 + c^2a^2 + d^2n^2}$, e denoting the angular velocity. But $\frac{f}{a^2} \times \sqrt{b^2m^2 + c^2a^2 + d^2n^2}$ is the initial momentum of rotation. Therefore, conſidering the momentum of rotation as invariable, the angular velocity will be invariable, e being always = f, which here denotes the initial angular velocity.

Our next buſineſs is to find the length of the track deſcribed by the momentary pole (P), upon the ſpherical ſurface; and the velocity of the pole in that track.

Fig. 2, 3. It appearing, that the motive force E is = $\frac{Mf^2}{3a^5} \times \sqrt{Dm^2 - Ca^2} \times \frac{\gamma}{g}$, and the motive force $\overset{\shortparallel}{E} = \frac{Mf^2}{3a^4g} \times \sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}$; we find $F = \sqrt{E^2 + \overset{\shortparallel}{E}^2}$ (the force compounded of thoſe two forces) = $\frac{Mf^2}{3a^5} \times \sqrt{D^2m^2n^2 - BCa^2\gamma^2}$; and, F being

to E as a to the sine of the angle pPu, it follows, that the sine of pPu will be $= \frac{\sqrt{Dm^2-Ca^2} \times a\gamma}{g\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$, and its cosine $= \frac{a^2\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}}{g\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$. Therefore, that cosine being to radius as $\left(\frac{a\gamma}{g}\right)$ the fluxion of the arc PQ to $(\dot{z})$ the fluxion of the polar track on the spherical surface, $\dot{z}$ will be $= \frac{\gamma\sqrt{D^2m^2n^2-BCa^2\gamma^2}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}}$. Now, $PpLN$ being a quadrant of a great circle (touching the said polar track at P), and NAN a diameter of that circle; if we put w to denote the distance of any particle (p) of the parallelopipedon from that diameter, and G to denote the accelerative force of any such particle when w is $= a$; the motive force F $\left(= \frac{Me}{3a^3} \times \sqrt{D^2m^2n^2-BCa^2\gamma^2}\right)$, computed above, will be $= \frac{G}{a^3} \times$ *the sum of all the* $w^2 \times p$; which sum, by computation, is found $= \frac{M}{3} \times \frac{d^2+b^2 \,.\, D^2m^2n^2-b^2m^2+c^2a^2+d^2n^2 \,.\, BC\gamma^2}{D^2m^2n^2-BCa^2\gamma^2}$. Consequently, G will be $= \frac{e^3}{a^3} \times \frac{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}{d^2+b^2 \,.\, D^2m^2n^2-b^2m^2+c^2a^2+d^2n^2 \,.\, BC\gamma^2}$. But, by what I have done in the *Philosophical Transactions* for the year 1777, $\frac{aG}{e}$ will be $= v =$ the velocity wherewith the momentary pole changes its place in the spherical surface to which it is referred. Therefore,

v will be $= \frac{e}{a^2} \times \frac{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}{d^2+b^2 \,.\, D^2m^2n^2-b^2m^2+c^2a^2+d^2n^2 \,.\, BC\gamma^2}$; and $\frac{\dot{z}}{v} = \dot{T}$, the fluxion of the time $= \frac{a^2\gamma}{e} \times \frac{d^2+b^2 \,.\, D^2m^2n^2-b^2m^2+c^2a^2+d^2n^2 \,.\, BC\gamma^2}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2} \times D^2m^2n^2-BCa^2\gamma^2}$

$=$

$$= \frac{b^2m^2+c^2a^2+d^2n^2}{e} \times \left\{ \frac{\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}} + \frac{Dm^2-Ca^2}{b^2m^2+c^2n^2+d^2n^2} \times \frac{D^2m^2n^2\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2} \times \overline{D^2m^2n^2-BCa^2\gamma^2}^{\frac{1}{2}}} \right\}$$: which,

when Dm^2 is $= Ca^2$, becomes $= \frac{d^2+b^2 \cdot a^2\dot{\gamma}}{e\sqrt{BC} \cdot \overline{a^2-\gamma^2}^{\frac{1}{2}}}$.

It is evident, that $\frac{d^2+b^2}{2e\sqrt{BC}} \times a \times$ hyp. log. of $\frac{a+\gamma}{a-\gamma}$, the value of T in that particular caſe, will be *infinite* when γ is $= a$; and this concluſion agrees with what is ſaid above reſpecting the motion of the momentary pole along the great circle CR (fig. 2. and 3.).

I have not found, that the value of T will, in general, be aſſigned by the *arcs of the conic ſections*; but my Tables * ſhew, that it will be ſo aſſigned when Dm^2 is $= Ba^2$, and in ſome other particular caſes.

We have ſtill to inveſtigate the track of the momentary pole in the *immoveable* concave ſpherical ſurface, which we muſt conceive to ſurround our *moveable* convex ſpherical ſurface, ſuppoſing the center of both thoſe ſurfaces to coincide with the centers of gravity of our parallelopipedon: which central point is always in this diſquiſition ſuppoſed at reſt.

Let AL be the projection of part of a great circle CL, at right angles to the great circle P*p*LN; then will the ſine of the arc CL be $= \frac{\sqrt{Dm^2-Ca^2} \times \gamma}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; its coſine $= \frac{D^{\frac{1}{2}}\sqrt{Da^2m^2n^2-Dm^4-Ca^2m^2+Ca^2\gamma^2 \cdot \gamma^2}}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; and, the fluxion of that ſine being $= \frac{\overline{Dm^2-Ca^2} \times D^2m^2n^2\dot{\gamma}}{\overline{D^2m^2n^2-BCa^2\gamma^2}^{\frac{3}{2}}}$, the fluxion of that arc (CL) will be

* *Mathematical Memoirs*, publiſhed in 1780.

=

$$= \frac{\sqrt{Dn^2 - Ca^2} \times D^{\frac{1}{2}} an^2 \dot{\gamma}}{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2} \times \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2} \cdot \gamma^2}.$$ Consequently,

the sine of the arc PL being $= \frac{a^2 \sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}} \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2} \cdot \gamma^2}$;

and this sine being to radius as the fluxion of the arc CL to the measure of the angle of contact of the polar track on the *moveable* spherical surface with a great circle, we find that measure

$$= \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{\gamma}}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2} \times \sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}} = \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{D^2 m^2 n^2 - BCa^2\gamma^2|^{\frac{1}{2}}}.$$

The measure of the angle of contact of the track of the momentary pole, in the *immoveable* spherical surface, with a great circle, will accordingly be

$$\dot{\iota}\dot{T} = \frac{\sqrt{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{D^2 m^2 n^2 - BCa^2\gamma^2|^{\frac{1}{2}}} = \frac{\sqrt{b^2 m^2 + c^2 n^2 + d^2 n^2} \cdot \dot{z}}{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}$$: by means of which

measure we may describe, by points, the track of the momentary pole in the spherical surface last mentioned.

There are other methods of finding that track; but I know none that is less difficult than this method, or in any respect more satisfactory.

The radius of the lesser circle, which is the circle of curvature of the polar track in our immoveable spherical surface, will be =

$$\frac{a\dot{z}}{\sqrt{\dot{z}^2 + \text{sq. of the meas. of the ang. of cont.}}} = \frac{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}{\sqrt{b^2 + c^2|^2 \cdot m^2 + c^2 + d^2|^2 \cdot n^2 - BC\gamma^2}}.$$

When B is = o, or very small in comparison with D, and Dm^2 is less than Ca^2, the last mentioned radius will be equal, or nearly equal, to the invariable quantity $\frac{Dmn}{\sqrt{b^2 + d^2|^2 \cdot m^2 + 4d^4 n^2}}$;

the track of the pole in the immoveable spherical surface being then exactly, or very nearly, a lesser circle. At the same time,

the

the polar track upon the moveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is m.

When C is $=0$, or very ſmall in compariſon with D, and Dm^2 is greater than Ca^2, the track of the pole in the immoveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is $=\frac{Dmn}{\sqrt{4c^4m^2+c^2+a^2}^2 \cdot n^2}$; and then the polar track upon the moveable ſpherical ſurface will be exactly, or very nearly, a leſſer circle whoſe radius is n.

Whatever the curves may be which the momentary pole ſhall deſcribe in thoſe two ſpherical ſurfaces, the track upon the moveable ſurface will always touch and roll along the track in the immoveable ſurface (whilſt the common center of both ſurfaces remains at reſt), in the manner deſcribed in my Paper in the *Philoſophical Tranſactions* for the year 1777; the velocity of the point of contact being equal to the value of v computed above, which velocity when B is $=0$, or very ſmall in compariſon with D, and Dm^2 is leſs than Ca^2, will be exactly, or very nearly, $=\frac{d^2-b^2}{d^2+b^2}\times\frac{mne}{a^2}$; and when C is $=0$, or very ſmall in compariſon with D, and Dm^2 is greater than Ca^2, that velocity will be exactly, or very nearly, $=\frac{d^2-c^2}{d^2+c^2}\times\frac{mne}{a^2}$.

The polar track upon the moveable ſpherical ſurface will always roll along the convexity of the track in the immoveable ſpherical ſurface; the convexity or concavity of the former being turned towards the convexity of the latter, according as Dm^2 is greater or leſs than Ca^2. Which track in the immoveable ſpherical ſurface, when it is not circular, will touch a certain circle as often as γ, during the motion, ſhall become $=0$; and likewiſe another parallel

circle

circle as often as γ ſhall become equal to $\left|\frac{\dot{C}}{C}\right|^{\frac{1}{2}} \times m$, or $\left|\frac{\dot{B}}{B}\right|^{\frac{1}{2}} \times n$; the parts of the track between the points of contact being perfectly ſimilar. If Dm^2 be $= Ca^2$ (Dn^2 being then $= Ba^2$, and conſequently $\frac{D}{C}\Big|^{\frac{1}{2}} \times m = \frac{D}{B}\Big|^{\frac{1}{2}} \times n = a$), the ſaid track will make an infinite number of revolutions about a certain point, continually approaching nearer and nearer thereto, without arriving thereat in any finite time, though the length of the ſpiral ſo deſcribed cannot exceed a certain finite qnantity.

M. Euler has computed, that if the motive forces to turn the revolving body about AB, AC, AD, be reſpectively denoted by H, I, K;

$$\text{H will be} = \frac{M}{3} \cdot \frac{d^2+e^2}{a^3\dot{T}} \times \text{flux. of } e\beta - \frac{M}{3a^5} \cdot Be^2\gamma\delta,$$

$$\text{I} \quad = \frac{M}{3} \cdot \frac{d^2+b^2}{a^3\dot{T}} \times \text{flux. of } e\gamma + \frac{M}{3a^5} \cdot De^2\beta\delta,$$

$$\text{K} \quad = \frac{M}{3} \cdot \frac{e^2+b^2}{a^3\dot{T}} \times \text{flux. of } e\delta - \frac{M}{3a^5} \cdot Ce^2\beta\gamma;$$

γ being ſuppoſed to decreaſe as T increaſes: and he has put the value of each of thoſe forces (H, I, K) $= 0$. In doing ſo, it ſeems to me, that he has erroneouſly aſſumed equations as generally true, which are only ſo in a particular caſe. For $\frac{M}{3a^5} \cdot Be^2\gamma\delta$ is the motive force to turn the body about AB, ariſing from the centrifugal force of its particles revolving about the momentary axis AP, ſuppoſing the pole to keep its place; and $\frac{M}{3} \cdot \frac{d^2+e^2}{a^3\dot{T}} \times$ flux. of $e\beta$ is the value of the motive force requiſite to cauſe the *whole* variation of the velocity $\left(\frac{e\beta}{a}\right)$ about AB. But the firſt mentioned force *alone* does not, in general,

cauſe *all* the variation of the velocity about AB; that velocity varies in conſequence of the evagation of the pole P; and that evagation is cauſed by the motive forces urging the body to turn about AB, AC, AD, *conjunctly*. Therefore the motive force $\frac{M}{3a^2} \cdot B\epsilon^2\gamma\delta$ about AB *only* will not, in general, be equal to $\frac{M}{3} \cdot \frac{d^2+\epsilon^2}{a^3\dot{T}} \times$ flux. of $\epsilon\beta$, the value of the *whole* motive force requiſite to cauſe the variation of the velocity $\frac{\epsilon\beta}{a}$, as M. EULER reckoned.

The like objection may, I conceive, be juſtly made to his other two equations ſimilar to that which is here particularly adverted to.

M. D'ALEMBERT's radical errors, in treating this ſubject, appear to me nearly ſimilar to M. EULER's.

Other arguments may be adduced to prove, that the equations aſſumed by thoſe gentlemen are not well founded. If the forces to turn the body about the lines AB, AC, AD were each = 0, the velocities about thoſe lines muſt each remain invariable; but it ſeems abſolutely impoſſible that they can ever remain ſo, whilſt the angles which thoſe lines make with the momentary axis are each continually varying. Moreover, according to their concluſions, the tangent at P to the track of polar evagation, upon the moveable ſpherical ſurface, will not always be perpendicular to the direction in which the pole P will be urged to turn by the joint centrifugal force of the particles of the revolving body; whereas it is proved, I preſume, beyond a doubt, in my Paper above-mentioned, that the ſaid track will always be interſected at right angles by the direction in which the momentary pole ſhall, at any inſtant of time, be urged to turn by the force cauſing its evagation.

If we resolve each of the three forces H, I, K, into two others; the one to turn the body about the diameter NAN, and the other to turn it about the momentary axis PAP, at right angles to that diameter; the forces to turn it in the last mentioned direction, arising from the said forces H, I, K, will be

$$\frac{\beta H}{a} = \frac{M}{3} \cdot \frac{d^2+e^2}{a^4 T} \times \beta \text{ flux. of } e\beta - \frac{M}{3a^6} \cdot Be^3\beta\gamma\delta,$$

$$\frac{\gamma I}{a} = \frac{M}{3} \cdot \frac{d^2+b^2}{a^4 T} \times \gamma \text{ flux. of } e\gamma + \frac{M}{3a^6} \cdot De^3\beta\gamma\delta,$$

$$\frac{\delta K}{a} = \frac{M}{3} \cdot \frac{c^2+b^2}{a^4 T} \times \delta \text{ flux. of } e\delta - \frac{M}{3a^6} \cdot Ce^3\beta\gamma\delta.$$

The sum of these forces, it is obvious, must be $=0$; the direction wherein they are supposed to act being at right angles to that in which the body will be actually urged to turn by the joint centrifugal force of its particles, and that being the only force whereby the motion of the body is supposed to be affected: which sum ($B+C-D$ being $=o$) is, when divided by $\frac{M}{3a^4T}$,

$$= \left\{ \begin{array}{l} \overline{d^2+c^2} \cdot \beta^2 \dot{e} + \overline{d^2+b^2} \cdot \gamma^2 \dot{e} + \overline{c^2+b^2} \cdot \delta^2 \dot{e} \\ \overline{d^2+c^2} \cdot e\beta\dot{\beta} + \overline{d^2+b^2} \cdot e\gamma\dot{\gamma} + \overline{c^2+b^2} \cdot e\delta\dot{\delta} \end{array} \right\} = o.$$

But $\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta}$ being before found $=o$, we have $\overline{d^2+c^2} \times \beta\beta+\gamma\gamma+\delta\delta = o$; and $b^2\beta\beta+c^2\gamma\gamma+d^2\delta\delta$ being also found $=o$; it evidently follows, that

$\overline{d^2+c^2} \cdot \beta\dot{\beta} + \overline{d^2+b^2} \cdot \gamma\dot{\gamma} + \overline{c^2+b^2} \cdot \delta\dot{\delta}$ will be $=o$.

Therefore $\overline{d^2+c^2} \cdot \beta^2\dot{e} + \overline{d^2+b^2} \cdot \gamma^2\dot{e} + \overline{c^2+b^2} \cdot \delta^2\dot{e}$ will be $=o$: consequently $\dot{e}$ will be $=o$, and e invariable; which agrees with what is said above respecting the momentum of rotation.

The other forces arising by resolution from the forces H, I, K, to turn the body about the diameter NAN, will be

$$-\frac{B\gamma H}{S} = -\frac{M}{3a^3}\cdot\frac{a^2+b^2}{ST}\cdot B\epsilon\gamma\delta\beta + \frac{M\epsilon^2}{3a^5S}\cdot B\gamma^2\delta^2,$$

$$\frac{D\beta H}{S} = \frac{M}{3a^3}\cdot\frac{a^2+b^2}{ST}\cdot D\epsilon\beta\delta\gamma + \frac{M\epsilon^2}{3a^5S}\cdot D^2\beta^2\delta^2,$$

$$-\frac{C\delta\gamma K}{S} = -\frac{M}{3a^3}\cdot\frac{a^2+b^2}{ST}\cdot C\epsilon\beta\gamma\delta + \frac{M}{3a^5S}\cdot C^2\beta^2\gamma^2;$$

$$S \text{ being} = \sqrt{D^2m^2n^2 - BC\alpha^2\gamma^2}.$$

And, no external force being ſuppoſed to act on the body, it follows, that the ſum of theſe three forces muſt be = 0: therefore we may infer, that

$$T \text{ will be} = \frac{a^2}{\epsilon}\times\frac{a^4-c^4\cdot\gamma\delta\beta-a^4-b^4\cdot\beta\delta\gamma+a^4-b^4\cdot\beta\gamma\delta}{b^2\gamma^2\delta^2+D^2\beta^2\delta^2+C^2\beta^2\gamma^2};$$ which agreeing with the value of T found above, the truth of our preceding proceſs is thus confirmed.

The force $\frac{M\epsilon^2}{3a^5S}\times\sqrt{B^2\gamma^2\delta^2+D^2\beta^2\delta^2+C^2\beta^2\gamma^2}$, ariſing from thoſe three forces, is the *whole* joint centrifugal force of the particles of the revolving body, to turn it about the diameter NAN the way it will actually be urged to turn by ſuch force; the value whereof ſo computed will be $\left(=\frac{M\epsilon^2}{3a^5S}\times\sqrt{D^2m^2n^2-BC\alpha^2\gamma^2}=\frac{M\epsilon^2S}{3a^5}\right)$ equal to the value of the force F computed above, both being conſidered as urging the body to turn in the ſame direction. And the quantity

$$\frac{M\epsilon^2}{3a^5ST}\times a^2+b^2-c^2\cdot\gamma\delta\beta-a^2+b^2\cdot D\beta\delta\gamma+a^2+b^2\cdot C\beta\gamma\delta$$

$\left(=\frac{\epsilon\dot{\alpha}}{\dot{t}^2}\times$ *the ſum of all the* $w^2\times\rho\right)$ is the value of the motive force which, acting in that very direction, is requiſite to cauſe the momentary pole to change its place as above deſcribed. This will ſee diſtinctly how the equation ariſes, by which the value of T is juſt now determined. I do

I do not find that the reſolving the forces H, I, K, in any other manner will conduce to the attainment of any uſeful concluſion.

It appears, by what is done above, that the forces

$$H \text{ is} = \frac{M\epsilon}{3a^3RT} \times CD \cdot m^2\beta^2\dot{\beta},$$

$$I \text{ is} = \frac{M\epsilon}{3a^3RT} \times BC \cdot \overline{Cn^2 - Bm^2} \cdot \gamma^2\dot{\gamma},$$

$$K = \frac{M\epsilon}{3a^3RT} \times BD \cdot n^2\delta^2\dot{\delta};$$

$$R \text{ being} = B^2\gamma^2\delta^2 + D^2\beta^2\delta^2 + C^2\beta^2\gamma^2.$$

And it is obvious, that each of the three laſt mentioned forces will be = o, if any two of the quantities b, c, d, be equal; two of the values of thoſe forces then vaniſhing, by reaſon of that equality; and the third value alſo vaniſhing by either $\dot{\beta}$, $\dot{\gamma}$, or $\dot{\delta}$, being at the ſame time = o. Therefore, in that caſe it happens, that M. Euler's computation agrees with mine: in every other caſe, I am clearly of opinion, his concluſions are not true. The ſame may be ſaid of M. D'Alembert's concluſions reſpecting the ſame propoſition.

The evagation of the pole of a revolving body conſidered above, does not ariſe from gravity, the attraction of any other body, or any external impulſe whatever; but is only the conſequence of the *inertia of matter*, and muſt neceſſarily enſue, according to the theory here explained, in every body in the univerſe, after having been made to revolve, without reſtraint, about any line paſſing through its center of gravity, that is not *a permanent* axis of rotation.

The Earth being neither uniformly denſe nor a perfect ſpheroid muſt, in ſtrictneſs, be conſidered as having only three permanent axes of rotation, agreeably to what I have proved in my *Mathematical Memoirs*; and it is diſturbed in its rotatory motion by the attraction of the ſun and moon (and other

other bodies in our ſyſtem); it follows; that it will not continually revolve about either of thoſe axes, but will revolve, or endeavour to revolve, about ſucceſſive momentary axes, as ſhewn above. If then its three permanent axes of rotation be called its *firſt*, *ſecond*, and *third* axes; and the poles of its *firſt* axis be thoſe about which its momentary poles are carried according to our theory; the *ſecond* and *third* axes will be in the plane of its equator, the three being at right angles to each other. Therefore, with reſpect to the above theory, this terreſtrial maſs muſt be conſidered of ſuch a form, that its equator, and any ſection parallel thereto, ſhall rather be elliptical than circular. And, denoting its firſt, ſecond, and third axes by b, c, d, reſpectively, obſervations evince, that the difference $c-b$ will be much greater than the difference $d-c$. Whence it follows, that (ſuppoſing the earth's rotatory motion to be diſturbed *only* by the centrifugal force ariſing from the *inertia* of its own particles) the track of polar evagation with us will be nearly circular, and the radius of the limiting circle very ſmall, whether we have regard to the moveable or immoveable ſpherical ſurface referred to above; but that, in the latter ſurface, ſuch circle will be much leſs than in the former: and it moreover follows, that the concavity of the track upon the moveable ſurface will continually touch and roll along the convexity of the track in the immoveable ſurface.

In other planets, the tracks of polar evagation may, from a ſimilar cauſe, be very different. The theory above explained evidently proves, that their axes of rotation may poſſibly vary greatly in poſition, merely through the *inertia of matter*; whilſt Providence has ſo ordered it, that the poſition of the axes of rotation of this planet ſhall, by that cauſe, be but very little altered.

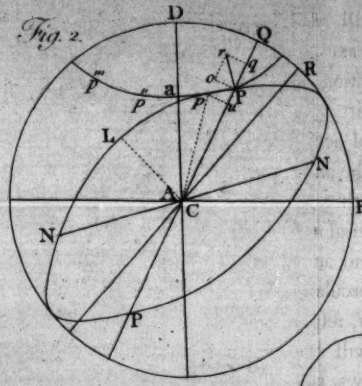


Fig. 2.
D
Q
R
L
N
A
C
B
P

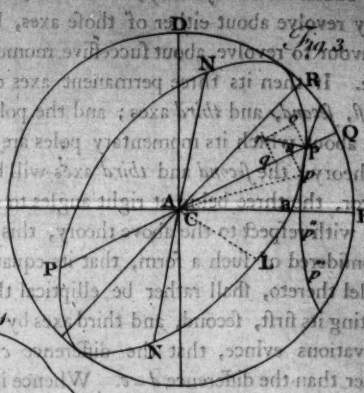


Fig. 3.
D
N
R
Q
A
C
B
P
L

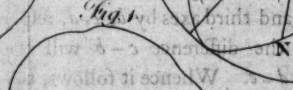


Fig. 1.
O
G
N

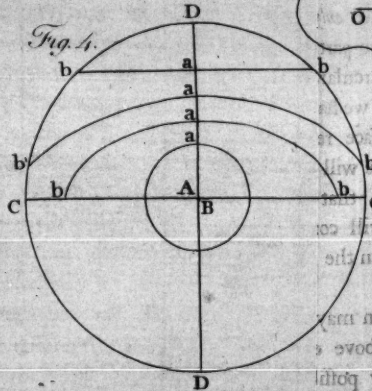


Fig. 4.
D
a
b
A
B
C

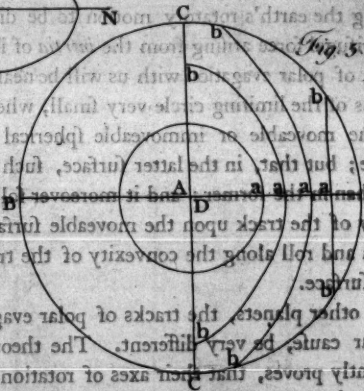


Fig. 5.
C
b
a
A
D
B

XVII. *Description of a new Marine Animal. In a Letter from Mr.* Everard Home, *Surgeon, to* John Hunter, *Esq. F.R.S. With a Postscript by Mr.* Hunter, *containing anatomical Remarks upon the same.*

Read March 7, 1785.

TO JOHN HUNTER, ESQ. F.R.S.

DEAR SIR, Sept. 20, 1784.

I SENT you, about three years ago, a sea animal from Barbadoes, which was unlike any one I had ever seen. From the want of books and other information in that island, I was unable at the time to find out, whether it was a new acquisition, or had been described by any authors in natural history.

Since my arrival in England, I have examined the libraries of some men of science for an account of this animal, and have made other enquiries among the naturalists, without success. The specimen I sent you was found on a part of the coast which had undergone very remarkable changes, in consequence of a violent hurricane. These changes were indeed the means of its being discovered, and present a probable reason why it was not discovered before. The extraordinary circumstances which brought it within our reach, and the silence of all the authors on natural history which I have been able to consult, incline me to believe it to be a non-descript. As the peculiarities of its structure may add to the knowledge of the natural history

hiſtory of other animals of this genus, at preſent ſo little underſtood, I have drawn out a more particular account of it; which, if you think it deſerves attention, you may preſent to the Royal Society.

This animal was found on the ſouth-eaſt coaſt of Barbadoes, cloſe to Charles Fort, about a mile from Bridge Town, in ſome ſhoal water, ſeparated from the ſea by the ſtones and ſand thrown up by the dreadful hurricane, which happened in the year 1780, and did ſo much miſchief to the iſland.

The wind, in the beginning of the ſtorm, which was in the afternoon, blew very furiouſly from the north-weſt, making a prodigious ſwell in the ſea; and in the middle of the night changing ſuddenly to the ſouth-eaſt, it blew from that quarter upon the ſea, already agitated, forcing it upon the ſhore with ſo much violence, that it threw down the rampart of Fort Charles, which was oppoſed to it, although thirty feet broad, by the burſting of one ſea. It forced up, at the ſame time, immenſe quantities of large coral rocks from the bottom of the bay, making a reef along this part of the coaſt for the extent of ſeveral miles, at only a few yards diſtance from the ſhore.

The ſoundings of the harbour were found afterwards to be intirely changed, by the quantity of materials removed from the bottom in different places. In the reef of coral was found an infinite number of large pieces of brain-ſtone, containing the ſhell of this animal; but the animals had either been long dead, or more probably deſtroyed by the motion of the rocks in the ſtorm: ſome few of the brain-ſtones, however, that had been thrown beyond the reef, and lodged in the ſhoal water, receiving leſs injury, the animals were preſerved unhurt.

The animal, with the ſhell, is almoſt intirely incloſed in the brain-ſtone, ſo that at the depth in which they generally lie,

they are hardly diſcernible, through the water, from the common ſurface of the brain-ſtone; but when in ſearch of food they throw out two cones, with membranes twiſted round them in a ſpiral manner, which have a looſe fringed edge, looking at the bottom of the ſea like two flowers; and in this ſtate they were diſcovered.

The ſpecies of Actinia called in Barbadoes the Animal Flower, and common to many parts of that iſland, although rarely before ſeen on this part of the coaſt, was now found in conſiderable numbers in this ſhoal water.

The animal was firſt obſerved by Captain Hendre, the officer commanding Fort Charles, in looking for ſhells which were thrown up in great numbers from the bottom of the harbour. He found a piece of brain-ſtone containing three of them in different parts of it. Some little time after, I was lucky enough to find another brain-ſtone with two in it; one of them is the ſpecimen in your poſſeſſion; the other was deſtined for examination, of which the following is the account.

The animal, when taken out of the ſhell, including the two cones and their membranes, is five inches in length; of which the body is three inches and three-quarters, and the apparatus for catching its prey, which may be conſidered as its tentacula, about an inch and a quarter.

The body of the animal is attached to its ſhell, for about three-quarters of an inch in length, at the anterior part where the two cones ariſe, by means of two cartilaginous ſubſtances, with one ſide adapted to the body of the animal, the other to the internal ſurface of the ſhell: the reſt of the body is unattached, of a darkiſh white colour, about half an inch broad, a little flattened, and rather narrower towards the tail. The muſcular fibres upon its back are tranſverſe; thoſe on the belly

longitudinal, making a band the whole length of the body, on the edge of which the tranſverſe fibres running acroſs the back terminate.

The two cartilaginous ſubſtances by which the animal adheres to its ſhell, are placed one on each ſide of the body, and are joined together upon the back of the animal at their poſterior edges: they are about three-quarters of an inch long, are very narrow at their anterior end, becoming broader as they go backwards; and at their poſterior end they are the whole breadth of the body of the animal. Upon their external ſurface there are ſix tranſverſe ridges, or narrow folds; and along their external edges, at the end or termination of each ridge, is a little eminence reſembling the point of a hair pencil, ſo that on each ſide of the animal there are ſix of theſe little projecting ſtuds, for the purpoſe of adhering to the ſides of the ſhell in which the animal is incloſed. The internal ſurfaces of theſe cartilages are firmly attached to the body of the animal, in their middle part, by a kind of band or ligament; but the upper and lower ends are lying looſe.

From the end of the body, between the two upper ends of theſe cartilages, ariſe what I ſuppoſe to be the tentacula, conſiſting of two cones, each having a ſpiral membrane twining round it: they are cloſe to each other at their baſes, and diverge as they riſe up, being about an inch and a quarter in length, and nearly one-ſixth of an inch in thickneſs at their baſe, and gradually diminiſhing till they terminate in points. The membranes which twine round theſe cones alſo take their origin from the body of the animal, and make five ſpiral turns and a half round each, being loſt in the points of the cones; they are looſe from the cone at the loweſt ſpiral turn which they make, and are nearly half an inch in breadth; they are exceed-

ingly

ingly delicate, and have at ſmall diſtances fibres running acroſs them from their attachment at the ſtem to the looſe edge, which gives them a ribbed appearance. Theſe fibres are continued about one-tenth of an inch beyond the membrane, having their edges finely ſerrated, like the tentacula of the Actiniæ found in Barbadoes: theſe tentacula ſhorten as the ſpiral turns become ſmaller, and are entirely loſt in that part of the membrane which terminates in the point of the cone.

Behind the origin of theſe cones ariſes a ſmall ſhell, which, for one-ſixth of an inch from its attachment to the animal, is very ſlender: it is about three quarters of an inch in length, becoming conſiderably broader at the other end, which is flat, and about one-third of an inch broad; the flattened extremity is covered with a kind of hair, and has riſing out of it two ſmall claws, about one-ſixth of an inch in length. If the hair, and mucus entangled in it, be taken away, this extremity of the ſhell becomes concave, is of a pink colour, and the two claws riſing out from its middle part have each three ſhort branches, not unlike the horns of a deer. The body of this ſhell has a ſoft cartilaginous covering, with an irregular but poliſhed ſurface: on this the cones reſt in their collapſed ſtate, in which ſtate the whole of the ſhell is drawn into the cavity of the brain-ſtone, excepting the flattened end with the two claws.

Before the cones there is a thin membrane, which appears to be of the ſame length with the ſhell juſt deſcribed. In the collapſed ſtate it lies between the cones and the ſhell in which the animal is incloſed; but, when the tentacula are thrown out, it is alſo protruded.

The ſhell of this animal is a tube, which is very thin, and adapted to its body: the internal ſurface is ſmooth, and of a

pinkiſh white colour: its outer ſurface is covered by the brain-ſtone in which it is incloſed, and the turnings and windings which it makes are very numerous. The end of the ſhell, which opens externally, riſes above the ſurface of the ſtone on one ſide half an inch in height, for about half the circumference of the aperture, bending a little forwards over it, and becoming narrower and narrower as it goes up, terminating at laſt in a point juſt over the center of the opening of the ſhell; on the other ſide it forms a round margin to the ſurface of the brain-ſtone. This part of the ſhell is much thicker and ſtronger than that part which is incloſed in the brain-ſtone: its outer ſurface is of a darkiſh brown colour; its inner of a pinkiſh white.

The animal, when at reſt, is wholly concealed in its ſhell; but when it ſeeks for food, the moveable ſhell is puſhed ſlowly out with the cones and their membranes in a collapſed ſtate; and when the whole is expoſed, the moveable ſhell falls a little back, and the membrane round each of the cones is expanded, the tentacula at the baſes of the cones having juſt room enough to move without touching one another. The thin membrane which lay between the cones and the incloſing ſhell is protruded in the form of a fold, and lies over the external ſhell which projects from the brain-ſtone.

The membranes have a ſlow ſpiral motion, which continues during the whole time of their being expanded; and the tentacula upon their edges are in conſtant action. The motion of the membrane of the one cone ſeems to be a little different from that of the other, and they change from the one kind of motion to the other alternately, a variation in the colour of the membrane at the ſame time taking place, either becoming a ſhade lighter or darker; and this change in the colour, while the

the whole is in motion, produces a pleasing effect, and is most striking when the sun is very bright. The membranes, however, at some particular times appear to be of the same colour.

While the membranes are in motion, a little mucus is often separated from the tentacula at the point of the cone. Upon the least motion being given to the water, the cones are immediately, and very suddenly, drawn in.

This apparatus for catching food is the most delicate and complicated that I have seen; but I shall not trouble you with any conjectures upon what that food may be, as I have not attained sufficient knowledge of the animal to speak with the smallest certainty.

I have endeavoured to describe the external appearances as I saw them; and have annexed two drawings of the animal in its two different states, one in search of food, and one while lying at rest; these are a little magnified, to show the parts more distinctly.

I shall not say any thing of the internal parts, or their uses, as the animal is in your possession, who are so much better able to explain its internal œconomy.

I am, &c.

EVERARD HOME.

POST-

POSTSCRIPT,

BY JOHN HUNTER, ESQ. F. R. S.

ANIMALS which come from foreign countries, and cannot be brought to England alive, muſt be kept in ſpirits to preſerve them from putrefaction, which makes them leſs fitted for anatomical examination; for the ſpirits, which preſerve them, produce a change in many of their properties, and alter the natural colours, and texture of the parts, ſo that often the ſtructure alone of the animal can be aſcertained; and where this is not naturally diſtinct, it becomes frequently intirely obſcured, and the texture of the finer parts is wholly deſtroyed, requiring a very extenſive knowledge of ſuch parts in animals at large, to aſſiſt us in bringing them to light: this happens to be the caſe with the animal whoſe diſſection is the ſubject of this Poſtſcript.

The animal may be ſaid to conſiſt of a fleſhy covering, a ſtomach and inteſtinal canal, and the two cones with their tentacula and moveable ſhell, which laſt may be conſidered as appendages.

The body of the animal is flattened, and terminates in two edges, which are interſected by rugæ, the faſciculi of tranſverſe muſcular fibres which run acroſs the back being continued over them. Upon each of theſe edges is placed a row of fine hairs, which project to ſome diſtance from the ſkin.

The fleſhy covering conſiſts principally of muſcular fibres: thoſe upon the back are placed tranſverſely, to contract the body

laterally; thoſe on the belly longitudinally, to ſhorten the animal when ſtretched out, and to draw it into the ſhell.

The ſtomach and inteſtine make one ſtraight canal: the anterior end of this forms the mouth, which opens into the grooves made by the ſpiral turns of the tentacula round the ſtem of each of the cones; and the inteſtine at the poſterior end opens externally, forming the anus. From the contracted ſtate of the animal, the inteſtine is thrown into a number of folds.

On examining the cones and the tentacula, I at firſt believed that the ſpiral form aroſe from their being in a contracted ſtate; and that, when the tentacula were erected, the cone untwiſted, forming a longer cone with the tentacula ariſing from its ſides, like the plume from the ſtem of a feather; and that this ſtem was drawn in or ſhortened by means of a muſcle paſſing along the center, which threw the tentacula into a ſpiral line, ſimilar to the penis's of many birds; but how far this is really the caſe, I have not been able to aſcertain.

The internal ſtructure of this animal, like moſt of thoſe which have tentacula, is very ſimple; it differs, however, materially from many, in having an anus, moſt animals of this tribe, as the Polypi, having only one opening, by which the food is received, and the excrementitious part of it alſo afterwards thrown out; this we muſt have ſuppoſed, from analogy, to take place in the animal which is here deſcribed, more particularly ſince it is incloſed in a hard ſhell, at the bottom of which there appears to be no outlet; but as there is an anus this cannot be the caſe.

It is very ſingular, that in the Leach, Polypi, &c. where no apparent inconvenience can ariſe from having an anus, there is not one, while in this animal, where it would ſeem to be attended with many, we find one; but there being no anus

in

in the Leach, Polypi, &c. may depend upon ſome circumſtance in the animal œconomy which we are at preſent not fully acquainted with.

The univalves, whoſe bodies are under ſimilar circumſtances reſpecting the ſhell with this animal, have the inteſtine reflected back, and the anus, by that means, brought near to the external opening of the ſhell, the more readily to diſcharge the excrement; and although this ſtructure, in theſe animals, appears to be ſolely intended to anſwer that purpoſe, yet when we find the ſame ſtructure in the black Snail, which has no ſhell, this reaſoning will not wholly apply, and we muſt refer it to ſome other intention in the animal œconomy.

In this animal we muſt therefore reſt ſatisfied that the diſadvantageous ſituation of the anus, with reſpect to the excrement's being diſcharged from the ſhell, anſwers ſome purpoſe in the œconomy of the animal, which more than counter-balances the inconveniences produced by it.

It would appear, from conſidering all the circumſtances, that the excrement thrown out at the anus muſt paſs from the tail along the inſide of the tube, between it and the body of the animal, till it comes to the external opening of the ſhell, as there is no other evident mode of diſcharging it.

How the tube or ſhell is formed in ſtone or coral is not eaſily aſcertained. It may be aſked, whether this animal has the power of boring backwards as the Teredo Navalis probably does, or whether the ſtone or coral is formed at the ſame time with the animal, and grows and increaſes with it: and if we conſider all the circumſtances, this laſt would appear to be moſt probable, and agree beſt with the different phænomena; for the coral is lined with a ſhell, which could not be the caſe if the animal was continually increaſing this hole, both in length and

breadth, in proportion to its growth; but if the coral and the animal increaſe together, it is then ſimilar to the growth of all ſhells, whether bivalve or univalve.

The animal does not appear to have the power of increaſing its canal, being only compoſed of ſoft parts. This, however, is no argument againſt its doing it, for every ſhell fiſh has the power of removing a part of its ſhell, ſo as to adapt the new and the old together; which is not done by any mechanical power, but by abſorption.

The tribe of animals which have tentacula conſiſts of an almoſt infinite variety, and many of the ſpecies have been deſcribed. Of that kind, however, which has the double cones, I believe hitherto no account has been given. It is moſt probably to be found in the ſeas ſurrounding the different iſlands in the Weſt Indies; for I received an animal, ſome years ago, from Mr. OLIVER, ſurgeon, at Tenby in Pembrokeſhire, which he had procured from a gentleman at St. Vincent's; which, upon examination, proves to be the ſame animal with that above deſcribed, only that the moveable ſhell is wanting.

Since I began this Poſtſcript, I find there is a deſcription of a double-coned Terebella, publiſhed by the rev. Mr. CORDINER, at Bamf in Scotland, which was found upon that coaſt; in which the cones have their tentacula paſſing out from the end, and when erected they ſpread from the cone as from a center. This proves that the double-coned tentacula alſo have different ſpecies.

EXPLANATION OF THE FIGURES, TAB. XI.

FIG. I.

A drawing of the animal after death, as it appeared in ſpirits, a little magnified.

A. The under ſide of the body.

BB. The cartilages which attach the animal to the ſides of the cavity in which it lies.

C. One of the cones covered by its membrane in a collapſed ſtate.

D. The loweſt ſpiral turn of the membrane and its tentacula ſpread out.

EE. The cut edges of the divided membrane, which are turned on each ſide to ſhew the cone.

F. The cone as it appears in the intervals between the ſpiral turns of the membrane.

G. The moveable ſhell, with the ſmooth cartilaginous covering, in an outſide view.

H. The flattened end of the moveable ſhell, with hair upon it.

II. The two claws that ariſe from the ſurface of the flattened end of the moveable ſhell.

K. The anus, into which a hog's briſtle is introduced.

FIG. II.

A drawing of the animal, with its tentacula expanded in ſearch of food, as it appears in the ſea; taken from a ſketch made in Barbadoes, where no draughtſman could be procured

Fig. I.

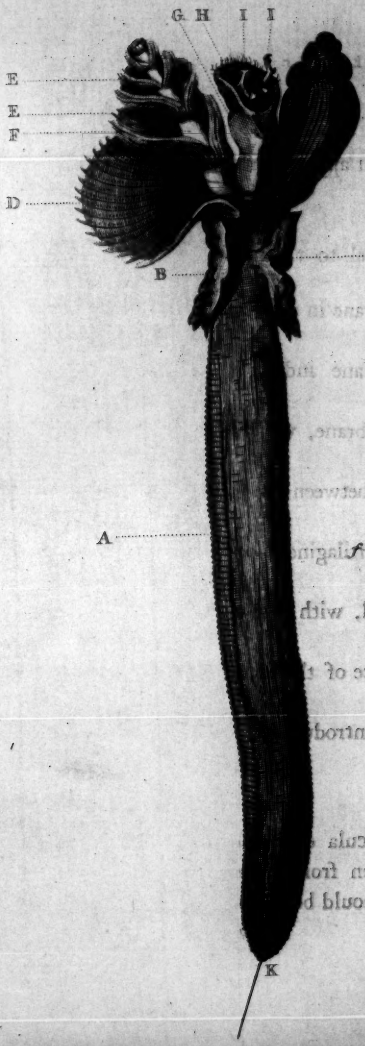
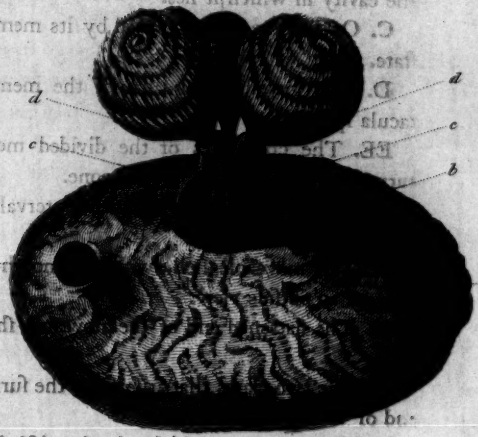


Fig. II.



while the animal was alive. This alſo is larger than the animal.

a. The ſort of brain-ſtone in which the animal was diſcovered.

b. The external prominent ſhell.

cc. The membrane which is protruded with the cones and moveable ſhell, and makes a fold over the edges of the prominent ſhell.

dd. The membranes and tentacula in a ſtate of expanſion.

e. The inner ſide of the moveable ſhell, as it appears when protruded.

f. The hole in the brain-ſtone as it appears when the prominent ſhell is broken off, and which may be ſeen in many ſpecimens of brain-ſtone.

XVIII. *A Description of a new System of Wires in the Focus of a Telescope, for observing the comparative Right Ascensions and Declinations of celestial Objects; together with a Method of investigating the same when observed by the Rhombus, though it happen not to be truly in an equatorial Position.* By the Rev. Francis Wollaston, *LL.B. F.R.S.*

Read April 7, 1785.

IN consequence of a paper communicated the last year to this Society, and honoured with a place in our Transactions, it may be expected of me, that I should now deliver in an account of what farther observations I have made on that constellation of which I then gave a rough map. This I readily would do, if they were in any degree worthy of the Society's notice. But as yet they are far from perfect: how much better they may succeed hereafter, time must shew.

Yet has this year perhaps not quite been lost; the difficulties which disappointed my hopes, having led to what appears to me an improvement in the instrument with which to pursue such observations.

My design, as was hinted in that Paper, was to ascertain, as well as I was able, the right ascensions and declinations of the stars I had laid down, by observing their *meridian passages* and *meridian altitudes*, where that could be done with such small instruments as mine; as also by their *comparative passages* through the field of an equatorial telescope furnished with a

ſyſtem of wires invented by Dr. BRADLEY, and called by the French *Reticule Rhomboïde*, whence it has commonly obtained in Engliſh the name of the Rhomboid.

In the former I was diſappointed by the weather; which from the time I went into the country, in the middle of May, till the end of June, when that conſtellation came to the meridian in the day-light, afforded me very few evenings fit for obſervation.

In the latter I failed, through the imperfection of my inſtrument, or my own want of ſkill in the uſe of it; for though a ſingle ſet of obſervations in any one evening would appear very good, yet when reduced by calculation, and confronted with other repeated trials, they never gave me the ſatisfaction I wiſhed.

The rhombus (for a rhombus, and not a rhomboid, it ought moſt properly to be called) is very good in theory; but very difficult to get executed with preciſion, and liable to ſome inaccuracy in the obſervation. The truth of it depends upon the longer diagonal being exactly twice the length of the ſhorter one; which requires an awkward angle (53° 7′ 48″) at the vertex, not eaſily to be hit by the workmen, and therefore ſeldom ſufficiently true. Beſide this, as the ſides of the rhombus, on which depends the calculation for differences of declination, are but 26° 33′ 54″ declining from the perpendicular or horary wire, a very ſmall error in obſerving the paſſage of a ſtar makes a very material difference in the reſult.

This determined me upon making trial of a ſquare placed angularly (an addition to M. CASSINI's wires at 45°, as may be ſeen in tab. XII. fig. 1.) which ſeems to anſwer better. I muſt confeſs I have not yet had opportunity for trying it ſo completely as I could wiſh: but I was unwilling to let this

year

year ſlip by, without making it known; ſince, I think, from what I have done with it, I may be confident of its utility *.

The *properties* and *advantages* of ſuch a ſyſtem of wires ſcarcely need to be pointed out to aſtronomers. The whole extent of the field is employed as it is in the rhombus (the want of which was ſaid to be Dr. BRADLEY's objection to M. CASSINI's wires); but being formed of right angles or half-right angles, to which workmen are moſt accuſtomed, they will always be apt to execute their part better; and the obliques, from the differences being juſt double to what they are in the rhombus, give the comparative declinations with twice the certainty. To this the number of correſponding obſervations in the paſſage of every ſtar add conſiderably; ſince you may calculate its diſtance from the center C, from the angle D or E, or from one of the intermediate angles K, as you ſhall ſee occaſion. The ſame indeed you may do in the rhombus from D or from E; or, if the rhombus be formed of wires, from the angle at L, fig. 2.; but only with half the preciſion. The reſult of a ſingle paſſage of any one ſtar (excepting towards the extremities of the field) gives the extent of the field equally in each, provided the declination of the ſtar be known, by deducing its diſtance from thoſe ſeveral angles; and ſuch deductions ſerve as a ſtill farther check upon every obſervation; be-

* What is here offered is by no means to be underſtood as recommending any ſyſtem of wires in preference to actual meaſurement with a micrometer, but to render the uſe of them as convenient as may be to ſuch gentlemen as are not provided with better inſtruments. The equatorial micrometer with a large field (ſuch as I have ſeen at Mr. ADAMS's, of Mr. SMEATON's conſtruction) I take to be the beſt inſtrument for taking differences of right aſcenſion and declination out of the meridian, and far ſuperior to any ſyſtem of fixed wires, or indeed to any equatorial ſector whatever.

cauſe,

cauſe, if any part of it be thought doubtful, its tallying or not tallying with the known extent of the field will ſhew whether there be any error, or where it lies. And, in each of them, the parallel wires will tell you whether the placing of your inſtrument be true or faulty; becauſe, if truly made and truly ſet, the ſame ſtar muſt take the ſame time in paſſing from one wire to its correſponding parallel; which will differ conſiderably, and in every ſtar the ſame way, if the poſition be faulty.

Some of theſe latter remarks might have been ſpared, but that they may ſerve as hints to ſuch gentlemen as may be inclined to lend their aſſiſtance to what was propoſed the laſt year, and who may not have conſidered the many helps to be derived from a croſs examination of the obſervations they make. For their uſe alſo it may be proper to add, what indeed is nothing new, that if the poſition of the inſtrument be found erroneous, the formula given by M. DE LA LANDE in his Aſtronomy will ſerve to rectify the obſervation. Calling the larger interval between the paſſage of any oblique and the horary wire m, and the ſmaller one n, $\frac{m^2 n + n^2 m}{m^2 + n^2}$ will give the difference of declination (in time, to be converted into degrees, and multiplied by the coſine of declination) from the angle where that oblique meets the horary; and $\frac{m^2 n - n^2 m}{m^2 + n^2}$ the difference in right aſcenſion from the ſame angle. It muſt ſurely be almoſt needleſs to mention, that where the poſition is true, *half* the interval of time between a ſtar's paſſing any two correſponding obliques, converted into degrees, and multiplied by the coſine of declination, will give the difference in declination of that ſtar from the angle where thoſe obliques meet, as the *whole* interval does in the rhombus.

But

But it may, perhaps, be of ſervice to aſtronomy, or at leaſt not unacceptable to thoſe gentlemen who uſe the Rhombus, that I ſhould ſubjoin another formula (contrived for me the laſt ſummer by my Son, now Mathematical Lecturer at Sidney College, Cambridge) for inveſtigating the comparative right aſcenſions and declinations of ſtars obſerved by it, when the inſtrument is not placed truly in the plane of the equator. I was led into wiſhing for ſome ſuch formula, in conſequence of an ingenious Paper, kindly communicated to me by Sir H. C. ENGLEFIELD, Bart. F. R. S. giving an account of his method of doing it by a ſcale and figure; which, though very eaſy when one is provided with ſuch a ſcale, appeared to me to be of leſs general uſe than by calculation; and I do not know that any thing of the kind is to be met with in any publication.

Let the angle DLL, fig. 2. (which, by conſtruction, is 63° 26′ 6″) be called a

The diagonal LL (whoſe extent, that is, what portion of a great circle it comprehends, muſt be known to the obſerver) be called - - - b

The larger interval obſerved between the paſſage of a ſtar by an oblique and the horary wire (as $b\,c$) - - m

The ſmaller ditto of the ſame ſtar (as $c\,d$) - - n

The larger ditto of another ſtar (as $\beta\,\gamma$) - - μ

The ſmaller ditto (as $\gamma\,\delta$) - - - ν

Then $\frac{2 \cdot \overline{m+n}}{m+n} \times$ tangent of the angle which LL makes with a parallel of declination; call this - q

The angle q being thus found, then

$\frac{2 \cdot \overline{m-n} \times \text{ſin. } \frac{a+q}{2}}{R \times \text{ſin. } a} \times$ cof. q. = difference in declination between the two points on the vertical wire where thoſe

those stars pass it. N. B. This being in *time* must be converted into *degrees*, and multiplied by cosine of declination as usual, to give the true difference in declination between the stars.

And the same expression, *viz.*

$\frac{2 \cdot n \sim, \times \text{sin.} \overline{a+q}}{R \times \text{sin.} a} \times \text{sin.} q =$ the difference in $\mathcal{R}$ between those two points; to be applied as a correction to the observed times.

The same may be done by the larger intervals m and μ, only by substituting $\overline{a-q}$ in the place of $\overline{a+q}$, thus:

$\frac{2 \cdot m \sim \mu \times \text{sin.} a-q}{R \times \text{sin.} a} \times \text{cos.} q =$ difference in declination as above;

or . . . . . $\times \text{sin.} q =$ ascensional difference.

If the stars differ too much in declination to come within the expression above (as N° 2. and 3.) then the differences of the angles D and E in declination and right ascension may be found thus:

$\frac{2 \cdot b \times \text{cos.} q}{R} =$ difference in declination between D and E;

$\frac{2 \cdot b \times \text{sin.} q}{R} =$ their ascensional difference;

and the difference of each star from its respectively nearest angle of the rhombus, may be deduced by the former expression, leaving out the consideration of the other star, thus:

$\frac{2 \cdot n \times \text{sin.} \overline{a+q}}{R \times \text{sin.} a} \times \text{cos.} q =$ difference of the star in declination from its nearest angle.

and . . . $\times \text{sin.} q =$ its difference in right ascension.

The application of these formulæ is very easy: for having o und q, if you set down its cosine in one column for declina-

tion, and its fine in another column for right aſcenſion, and under each the conſtant fin. $\overline{a+q}$, and the arithmetical compl. of fin. a; theſe being added together will make two ſums, for the comparative obſervations of every ſtar which may paſs your field; and, unleſs your field be very large, and the declination of the ſtars very great, if to the column for declination you add the coſine of declination of the center of your field, it will adapt itſelf to all the products.

FRANCIS WOLLASTON.

Charter-houſe-Square,
March 15, 1785.

POSTSCRIPT.

SINCE the delivery of this Paper, it has occurred to me, that it may ſometimes be convenient to know the angle of deviation from the true equatorial poſition in the new ſyſtem of wires. This is to be deduced nearly in the ſame manner as in the rhombus; for $\frac{m-n}{m+n}$ = tang. q. By this angle any obſerved differences in right aſcenſion may be corrected: for the difference in declination between any two ſtars (or their difference from the angle) multiplied by fin. q, will give the correction required.

Fig. 1.

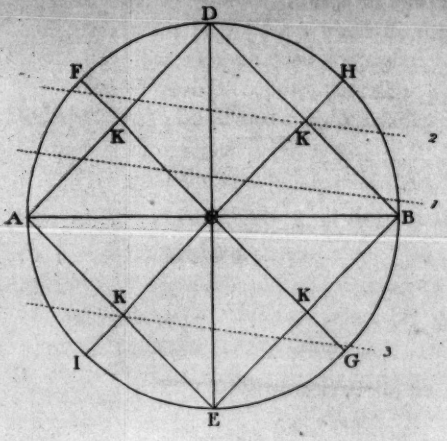
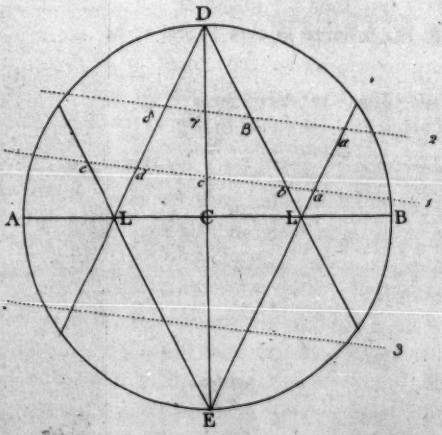


Fig. 2.



XIX. *An Account of a Stag's Head and Horns, found at* Alport, *in the Parish of* Youlgreave, *in the County of* Derby. *In a Letter from the Rev.* Robert Barker, *B.D. to* John Jebb, *M.D. F.R.S.*

Read April 14, 1785.

ABOUT five years ago, some men working in a quarry of that kind of stone which in this part of the country we call tuft *, at about five or six feet below the surface, in a very solid part of the rock, met with several fragments of the horns and bones of one or different animals. Amongst the rest, out of a large piece of the rock, which they got entire, there appeared the tips of three or four horns, projecting a few inches from it, and the scapula of some animal adhering to the outside of it. A friend of mine, to whom the quarry belongs, sent the piece of the rock to me in the state they got it, in which I let it remain for some time. But suspecting that they might be tips of the horns of some head enclosed in the lump, I determined to gratify my curiosity by clearing away the stone from the horns. On doing which I found that the lump contained a very large stag's head, with two antlers upon each horn, in very perfect preservation, inclosed in it.

* Tuft is a stone formed by the deposit left by water passing through beds of sticks, roots, vegetables, &c. of which there is a large stratum at Matlock Bath, in this county.

Though the horns are ſo much larger than thoſe of any ſtag I have ever ſeen, yet, from the ſutures in the ſkull appearing very diſtinct in it, one would ſuppoſe that it was not the head of a very old animal. I have one of the horns nearly entire, and the greateſt part of the other, but ſo broken in the getting out of the rock, that one part will not join to the other, as the parts of the other horn do. The horns are of that ſpecies which park-keepers in this part of the country call throſtle-neſt horns, from the peculiar formation of the upper part of them, which is branched out into a number of ſhort antlers which form an hollow about large enough to contain a thruſh's neſt. I ſend you the dimenſions of the different parts of them, compared with the horns of the ſame ſpecies of a large ſtag, which have probably hung in the place from whence I procured them two or three or perhaps more centuries; and with another pair of horns of a different kind, which are terminated by one ſingle pointed antler, and which were the horns of a ſeven-year-old ſtag.

The river Larkell runs down the valley, and part of it falls into the quarry where theſe horns were found, the water of which has not the property of incruſting any bodies it paſſes through. It is therefore probable, that the animal to which theſe horns belonged was waſhed into the place where they were found, at the time of ſome of thoſe convulſions which contributed to raiſe this part of the iſland out of the ſea. Beſides this complete head, I have ſeveral pieces of horns, bones (particularly the ſcapula I mentioned above), and ſeveral vertebræ of the back, found in the ſame quarry; ſome, if not all, of them probably belonging to the animal whoſe head is in my poſſeſſion.

Dimen-

Dimensions of the horns found at Alport.

	Ft.	In.
Circumference at their insertion into the corona,	0	9⅞
Length of the lowest antler,	1	2
Length of second ditto,	0	11½
Length of third ditto,	1	1½
Length of the horn,	3	3½

Dimensions of a large pair of throstle-nest horns.

Circumference at their insertion into the corona,	0	7
Length of the lowest antler,	1	0
Length of second ditto,	0	10½
Length of third ditto,	0	11½
Length of the horn,	2	7½

Dimensions of the horns of a stag seven years old.

Circumference at their insertion into the corona,	0	5½
Length of the lowest antler,	0	9
Length of second ditto,	0	10
Length of third ditto,	0	10
Length of the horn,	2	8½

Youlgreave, Jan. 23, 1785.

XX. *An Account of the ſenſitive Quality of the Tree* Averrhoa Carambola. *In a Letter from* Robert Bruce, *M.D. to Sir* Joſeph Banks, *Bart. P.R.S.*

Read April 14, 1785.

THE Averrhoa Carambola of LINNÆUS, a tree called in Bengal the Camruc or Camrunga, is poſſeſſed of a power ſomewhat ſimilar to thoſe ſpecies of Mimoſa which are termed ſenſitive plants; its leaves, on being touched, move very perceptibly.

In the Mimoſa the moving faculty extends to the branches; but, from the hardneſs of the wood, this cannot be expected in the Camrunga. The leaves are alternately pinnated, with an odd one; and in their moſt common poſition in the day-time are horizontal, or on the ſame plane with the branch from which they come out. On being touched, they move themſelves downwards, frequently in ſo great a degree that the two oppoſite almoſt touch one another by their under ſides, and the young ones ſometimes either come into contact or even paſs each other.

The whole of the leaves of one pinna move by ſtriking the branch with the nail of the finger, or other hard ſubſtance; or each leaf can be moved ſingly, by making an impreſſion that ſhall not extend beyond that leaf. In this way, the leaves of one ſide of the pinna may be made to move, one after another, whilſt the oppoſite continue as they were; or you may make them

them move alternately, or, in ſhort, in any order you pleaſe, by touching in a proper manner the leaf you wiſh to put in motion. But if the impreſſion, although made on a ſingle leaf, be ſtrong, all the leaves on that pinna, and ſometimes on the neighbouring ones, will be affected by it.

What at firſt ſeemed ſurpriſing was, that notwithſtanding this apparent ſenſibility of the leaf, I could with a pair of ſharp ſciſſars make large inciſions in it, without occaſioning the ſmalleſt motion; nay, even cut it almoſt entirely off, and the remaining part ſtill continue unmoved; and that then, by touching the wounded leaf with the finger or point of the ſciſſars, motion would take place as if no injury had been offered. But, on further examination, I found, that although the leaf was the oſtenſible part which moved, it was in fact entirely paſſive, and that the petiolus was the ſeat both of ſenſe and action: for although the leaf might be cut in pieces, or ſqueezed with great force, provided its direction was not changed, without any motion being occaſioned; yet, if the impreſſion on the leaf was made in ſuch a way as to affect the petiolus, the motion took place. When, therefore, I wanted to confine the motion to a ſingle leaf, I either touched it ſo as only to affect its own petiolus, or, without meddling with the leaf, touched the petiolus with any ſmall-pointed body, as a pin or knife.

By compreſſing the univerſal petiolus near the place where a partial one comes out, the leaf moves in a few ſeconds, in the ſame manner as if you had touched the partial petiolus.

Whether the impreſſion be made by puncture, percuſſion, or compreſſion, the motion does not inſtantly follow; generally ſeveral ſeconds intervene, and then it is not by a jirk, but

regular

regular and gradual. Afterwards, when the leaves return to their former ſituation, which is commonly in a quarter of an hour or leſs, it is in ſo ſlow a manner as to be almoſt imperceptible.

On ſticking a pin into the univerſal petiolus at its origin, the leaf next it, which is always on the outer ſide, moves firſt; then the firſt leaf on the oppoſite ſide, next the ſecond leaf on the outer, and ſo on. But this regular progreſſion ſeldom continues throughout; for the leaves on the outer ſide of the pinna ſeem to be affected both more quickly, and with more energy, than thoſe of the inner, ſo that the fourth leaf on the outer ſide frequently moves as ſoon as the third on the inner; and ſometimes a leaf, eſpecially on the inner ſide, does not move at all, whilſt thoſe above and below it are affected in their proper time. Sometimes the leaves at the extremity of the petiolus move ſooner than ſeveral others which were nearer the place where the pin was put in.

On making a compreſſion with a pair of pincers on the univerſal petiolus, between any two pair of leaves, thoſe above the compreſſed part, or nearer the extremity of the petiolus, move ſooner than thoſe under it, or nearer the origin; and frequently the motion will extend upwards to the extreme leaf, whilſt below it perhaps does not go farther than the neareſt pair.

If the leaves happen to be blown by the wind againſt one another, or againſt the branches, they are frequently put in motion; but when a branch is moved gently, either by the hand or the wind, without ſtriking againſt any thing, no motion of the leaves takes place.

When left to themſelves in the day-time, ſhaded from the ſun, wind, rain, or any diſturbing cauſe, the appearance of

the leaves is different from that of other pinnated plants. In the laſt a great uniformity ſubſiſts in the reſpective poſition of the leaves on the pinna; but here ſome will be ſeen on the horizontal plane, ſome raiſed above it, and others fallen under it; and in an hour or ſo, without any order or regularity, which I could obſerve, all theſe will have changed their reſpective poſitions. I have ſeen a leaf, which was high up, fall down: this it did as quickly as if a ſtrong impreſſion had been made on it; but there was no cauſe to be perceived.

Cutting the bark of the branch down to the wood, and even ſeparating it about the ſpace of half an inch all round, ſo as to ſtop all communication by the veſſels of the bark, does not for the firſt day affect the leaves, either in their poſition or their aptitude for motion.

In a branch, which I cut through in ſuch a manner as to leave it ſuſpended only by a little of the bark no thicker than a thread, the leaves next day did not riſe ſo high as the others; but they were green and freſh, and, on being touched, moved, but in a much leſs degree than formerly.

After ſun-ſet the leaves go to ſleep, firſt moving down, ſo as to touch one another by their under ſides; they therefore perform rather more extenſive motion at night of themſelves than they can be made to do in the day-time by external impreſſions. With a convex lens I have collected the rays of the ſun on a leaf, ſo as to burn a hole in it, without occaſioning any motion. But when the experiment is tried on the petiolus, the motion is as quick as if from ſtrong percuſſion, although the rays were not ſo much concentrated as to cauſe pain when applied in the ſame degree on the back of the hand; nor had the texture of the petiolus been any ways changed by this; for next day it

could not be distinguished, either by its appearance or moving power, from those on which no experiment had been made.

The leaves move very fast from the electrical shock, even although a very gentle one; but the state of the atmolphere was so unfavourable for experiments of this kind, that I could not pursue them so far as I wished.

There are two other plants mentioned as species of this genus by LINNÆUS. The first, the Averrhoa Bilimbi, I have not had an opportunity of seeing. The other, or Averrhoa Acida, does not seem to belong to the same class; nor do its leaves possess any of the moving properties of the Carambola. LINNÆUS's generic description of the Averrhoa, as of many other plants in this country which he had not an opportunity of seeing fresh, is not altogether accurate. The petals are connected by the lower part of the lamina, and in this way they fall off whilst the ungues are quite distinct. The stamina are in five pairs, placed in the angles of the germen. Of each pair only one stamen is fertile, or furnished with an anthera. The filaments are curved, adapted to the shape of the germen. They may be pressed down gently, so as to remain; and then, when moved a little upwards, rise with a spring. The sertile are twice the length of those destitute of antheræ.

Calcutta, Nov. 23, 1783.

XXI. *An Account of some Experiments on the Loss of Weight in Bodies on being melted or heated. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read April 28, 1785.

SIR,

ALTHOUGH I have made many experiments on the subject of the loss of weight in bodies on being melted, or heated, I do not think it worth while to lay them all before the Society, as there has not appeared any circumstance of contradiction in them. I shall content myself with relating the following one, which appears to me conclusive in determining the loss of weight in ice when thawed into water, and subject to the least fallacy of any I have hitherto made, in shewing the loss of weight in ice on being heated.

The beam I made use of was so adjusted as that, with a weight between four and five ounces in each scale, $\frac{1}{1000}$ part of a grain made a difference of one division on the index. It was placed in a room, the heat of which was 37 degrees of FAHRENHEIT's thermometer, between one and two in the afternoon, and left till the whole apparatus and the brass weights acquired the same temperature.

A glass globe, of three inches diameter nearly, with an indentation at the bottom, and a tube at the top, weighing about 451 grains, had about 1700 grains of *New-River* water poured into it, and was hermetically sealed, so that the

whole, when perfectly clean, weighed 2150$\frac{11}{14}$ of a grain exactly; the heat being brought to 32 degrees, by placing it in a cooling mixture of salt and ice till it just began to freeze, and shaking the whole together.

After it was weighed it was again put into the freezing mixture, and let stand for about twenty minutes; it was then taken out of the mixture; part of the water was found to be frozen; and it was carefully wiped, first with a dry linen cloth, and afterwards with dry washed leather; and on putting it into the scale it was found to have gained about the $\frac{1}{100}$ part of a grain. This was repeated five times: at each time more of the water was frozen, and more weight gained. In the mean time the heat of the room and apparatus had sunk to the freezing point.

When the whole was frozen, it was carefully wiped and weighed, and found to have gained $\frac{7}{100}$ of a grain and four divisions of the index. Upon standing in the scale for about a minute, I found it began to lose weight, on which I immediately took it out, and placed it at a distance from the beam. I also immediately plunged a thermometer in the freezing mixture, and found the temperature 10 degrees; and on putting the ball of the thermometer in the hollow at the bottom of the glass vessel, it shewed 12 degrees. I left the whole for half an hour, and found the thermometer, applied to the hollow of the glass, at 32°. Every thing now being at the same temperature, I weighed the glass containing the ice, after wiping it carefully, and found it had lost $\frac{1}{7}$ and five divisions; so that it weighed $\frac{11}{14}$, all but one division, more than when the water was fluid.

I now melted the ice, excepting a very small quantity, and left the glass vessel exposed to the air in the temperature of 32 degrees

degrees for a quarter of an hour; the little bit of ice continued nearly the same. I now weighed it, after carefully wiping the glass, and found it heavier than the water was at first one division of the beam. Lastly, I took out the weights, and found the beam exactly balanced as before the experiment.

The acquisition of weight found on water's being converted into ice, may arise from an increase of the attraction of gravitation of the matter of the water; or from some substance imbibed through the glass, which is necessary to render the water solid.

Which of these positions is true may be determined, by forming a pendulum of water, and another of ice, of the same length, and in every other respect similar, and making them swing equal arcs. If they mark equal times, then certainly there is some matter added to the water. If the pendulum of ice is quicker in its vibrations, then the attraction of gravitation is increased. For there is no position more certain, than that a single particle of inanimate matter is perfectly incapable of putting itself in motion, or bringing itself to rest; and therefore, that a certain force applied to any mass of matter, so as to give it a certain velocity, will give half the quantity of matter double the velocity, and twice the quantity, half the velocity; and, generally, a velocity exactly in the inverse proportion to the quantity of matter. Now, if there be the same quantity of matter in water as there is in ice, and if the force of gravity in water be $\frac{1}{28000}$ part less than in ice, and the pendulum of ice swing seconds, the pendulum of water will lose $\frac{1}{14000}$ of a second in each vibration, or one second in 28000, which is almost three seconds a day, a quantity easily measured.

I shall just take notice of an opinion which has been adopted by some, that there is matter absolutely light, or which repels instead of attracting other matter. I confess this appears absurd to me; but the following experiment would prove or disprove it. Supposing, for instance, that heat was a body, and absolutely light, and that ice gained weight by losing heat; then a pendulum of ice would swing through the same arc in $\frac{1}{12000}$ less time than a similar pendulum of water; for the same power would not only act upon a less quantity of matter, but a counter-acting force would also be taken away.

Till the experiment of the pendulum can be made, or some other equally certain be suggested and made, it would be wasting time to enter into conjecture about the cause of the gain of weight in the conversion of water into ice in a glass vessel hermetically sealed.

I shall only observe, that heat certainly diminishes the attractions of cohesion, chemistry, magnetism, and electricity; and if it should also turn out, that it diminishes the attraction of gravitation, I should not hesitate to consider heat as the quality of diminution of attraction, which would in that case account for all its effects.

We come, in the next place, to take notice of the second part of the experiment, *viz.* that the ice gained an eighth part of a grain on being cooled to 12 degrees of FAHRENHEIT's thermometer. In this case, a variation may arise from the contraction of the glass vessel, and consequent increase of specific gravity in proportion to the air. But it is unnecessary to observe, that this would be so very small a quantity as not to be observable upon a beam adjusted only to the degree of sensibility with which this experiment was tried. In the second place, the air cooled by the ice above the scale becoming heavier than

the ſurrounding atmoſphere, would preſs upon the ſcale downward with the whole force of the difference. If a little more than half a pint of air was cooled over the ſcale to the heat of the ice and glaſs containing it, that is, twenty degrees below the freezing point, the difference, according to General Roy's table, would have been the eighth part of a grain, which was the weight acquired; but the air within half an inch of the glaſs veſſel being only one degree below the freezing point, I cannot conceive, that even an eighth part of a pint of air could be cooled over the ſcale to twenty degrees below the freezing point; nor that the whole difference of the weight of the air over the ſcale could ever amount to the 32d of a grain. I have, however, contrived an apparatus which is executing, in which this cauſe of fallacy will be totally removed. I ſhall, therefore, reſt at preſent the ſtate of this part of the ſubject; and leave it only proved, that water gains weight on being frozen.

I am, &c.

G. FORDYCE.

XXII. *Sketches and Descriptions of three simple Instruments for drawing Architecture and Machinery in Perspective.* By Mr. James Peacock; *communicated by* Robert Mylne, *Esq.* F.R.S.

Read March 17, 1785.

SOME of the following machines must be placed upon the front edge of the table upon which they are to stand.

The sights may be supported by a three-legged staff.

The stocks of the squares or indexes may have steel springs upon their edges, in order to keep them in any assigned part of the grooves in which they are to slide.

FIG. I. (TAB. XIII.)

ABCD a drawing board, to be fixed on a table or stand, &c. in a vertical position. AB a sliding-piece for the top of the T square, having a rebate therein to form a groove, as expressed by the dotted line. CD, sliding-piece for the bottom of the square, having a rebate therein to form a groove for the reception of the stock as described by the dotted line; this sliding-piece to be of sufficient length to receive and support the said stock when the blade of the square is coincident with the lines KNFH or LNGI. E a hole to receive the arm or slider of the sight-piece, to be constructed in the usual manner. FGHI an opening forming the field of view for the prototype. KLMN a sheet of paper fixed on the upper part of the board for the copy, the four inner lines whereof form and inclose a space of the

the same dimensions as the field of view respectively. OP a steel sliding-piece, equal in length to the distance KF or IN; at the lower end P is a steel arm terminating in a point; and at the upper end, at O, is a similar arm, terminating with a brass button, in the center of which is a sharp steel pricker; the said pricker and the point P are to be equi-distant from the edge of the blade of the square: this arm O is to have the faculty of a spring, in order that the pricker may clear itself of the surface of the paper as soon as the finger quits the button, in the same manner as is usual in the apparatus of large protractors. This sliding steel-piece may be drawn out of the dove-tailed or rebated groove at pleasure, and the T square will then be fit for ordinary uses.

To use the Instrument.

Having fixed the board truly level and perpendicular, and placed the point of sight, or hole for vision, at such a height and distance as shall be productive of the best effect, move the square with one hand, and the steel slider with the other, until the point P coincides with the eye and any point or angle in the original object. Press the pricker at O, and the puncture will be the true place, or copy, of such original point or angle, &c.

N. B. All perpendicular lines may be drawn at once (in pencil), by bringing the left-hand edge of the square to coincide with the original line and the eye; and their lengths may be very nearly determined by the graduated edge of the square, so as to prevent confusion from unnecessary lengths of lines. The said graduated edge will also give the points in all curved or irregular objects.

FIG. II. (TAB. XIV.)

As the inſtrument, fig. 1. proceeds chiefly by finding the poſitions of Points, this is contrived to find the poſitions of Lines, and to determine their limits by their reciprocal interſections.

ABOCDE is a compound board, to be placed in a vertical poſition. FGHI is the opening, or field of view. KLMN is a looſe board, upon which paper is to be fixed; and the edges of the ſaid board are to be rebated, as deſcribed in the plan at *zz*. XYMN and OPQR are grooved receſſes, to receive the ſaid looſe board, as occaſion may require. STUW is a moveable parallelogram, compoſed of a rebated ſtock SU, two like-graduated rulers ST and UW, and the regulating piece TW; the whole connected with ſcrews, ſo as to move freely with a ſmall force; and the diſtances between the centers of motion SU or TW to be equal to KF or HQ. AE and ED are rebated grooves, in which the ſtock of the parallelogram is to move.

To uſe the Inſtrument.

Having fixed the compound board ABOCDE truly vertical, ſlip the papered board KLMN into the receſs XYMN, or OPQR, as the ſubject to be drawn may render firſt neceſſary, and ſlide the ſtock SU of the parallelogram into the groove AE, or ED, to correſpond therewith; then, by moving the ſtock in the groove with one hand, and at the ſame time regulating the parallelogram with the other, the top edge of the ruler UW may be brought to coincide with any line in the original object, and the figured diviſions on the edge of the ruler will at the ſame time determine the limits thereof, near enough to avoid a confuſion of unneceſſary lengths of line, &c. The true repre-

ſentation of the place and poſition of the line may be then drawn upon the paper, by the top edge of the ruler ST, a trifle longer at each extremity than it appears to be. This operation may be repeated for as many lines as can be obtained in the firſt poſition of the papered board and parallelogram; when they muſt be ſhifted into the other receſs and groove, to find the reſt, which may be now done without taking any further notice of the diviſions on the rulers.

N. B. A common T ſquare, applied to a board of this kind, will anſwer moſt purpoſes. For example: place the ſtock of ſuch a ſquare in one of the grooves, having a blade not leſs than the length HK or HR; mark the ſpaces HI and QR upon the upper edge thereof, and divide each of them into any convenient number of equal parts, and figure the ſaid parts in the uſual manner, to correſpond with each other, as may be ſeen in fig. 1. Now, ſuppoſe the ſtock of the ſquare to be in ED, it is plain, that all perpendicular lines may be drawn upon the paper KLMN in their proper places, and (by means of the diviſions on the edge of the ſquare) nearly of (though properly a trifle more than) their true length. All the lines of this deſcription being obtained, the ſhifting board muſt be placed in its other receſs, and the ſtock of the ſquare into the other groove; then, beginning with the firſt line, bring the edge of the ſquare to agree with its limits, and mark them off upon the line on the paper, and ſo of all the reſt in ſucceſſion; and join the points, where neceſſary, with a common ruler.

FIG. III. (TAB. XIV.)

This apparatus is contrived to avoid the trouble of continually working againſt a board in a vertical poſition. In order

to this, two ſquare boards are to be provided, equal in ſize, and of ſimilar conſtruction; one is to be fixed in a vertical poſition, for viewing the original object through a proper aperture; and the other is to be laid flat upon a deſk or table, for the greater eaſe and conveniency of drawing the copy upon paper to be fixed thereon for that purpoſe.

ABCD is the vertical board; EFGH the opening therein, forming the field of view; IKL the T ſquare, the blade thereof PL being moveable about the center P, with a moderate degree of ſtiffneſs; the ſtock K is to ſlide in a rebated or dove-tail groove AD, and be fixable to any part thereof by the ſcrew O; the ſteel points MN are to move with moderate eaſe in a rebated or dove-tail braſs groove in the middle of the blade of the ſquare; upon the back of the groove AD are to be fixed two braſs pins QQ, to reſt in proper holes, ſimilar to the holes marked RR; and the ſame kind of holes are to be made in the corner of the board whereon the copy is to be made.

To uſe the Machine.

Having placed the board ABCD in a truly vertical poſition, fix the ſhifting groove AD in the rebate, on the moſt convenient ſide of the board, by means of entering the pins Q into the holes R; then looſen the ſcrew O, and move the ſtock IK, and at the ſame time turn the blade PL upon its center P, until one of its edges ſhall be coincident with ſome original line; then fix the ſtock by turning the ſcrew O; move the points M and N, till they exactly include the apparent length of the ſaid line; then take off the ſhifting groove AD, together with the T ſquare or bevil fixed thereto, and apply the ſame to the

Fig. 1.

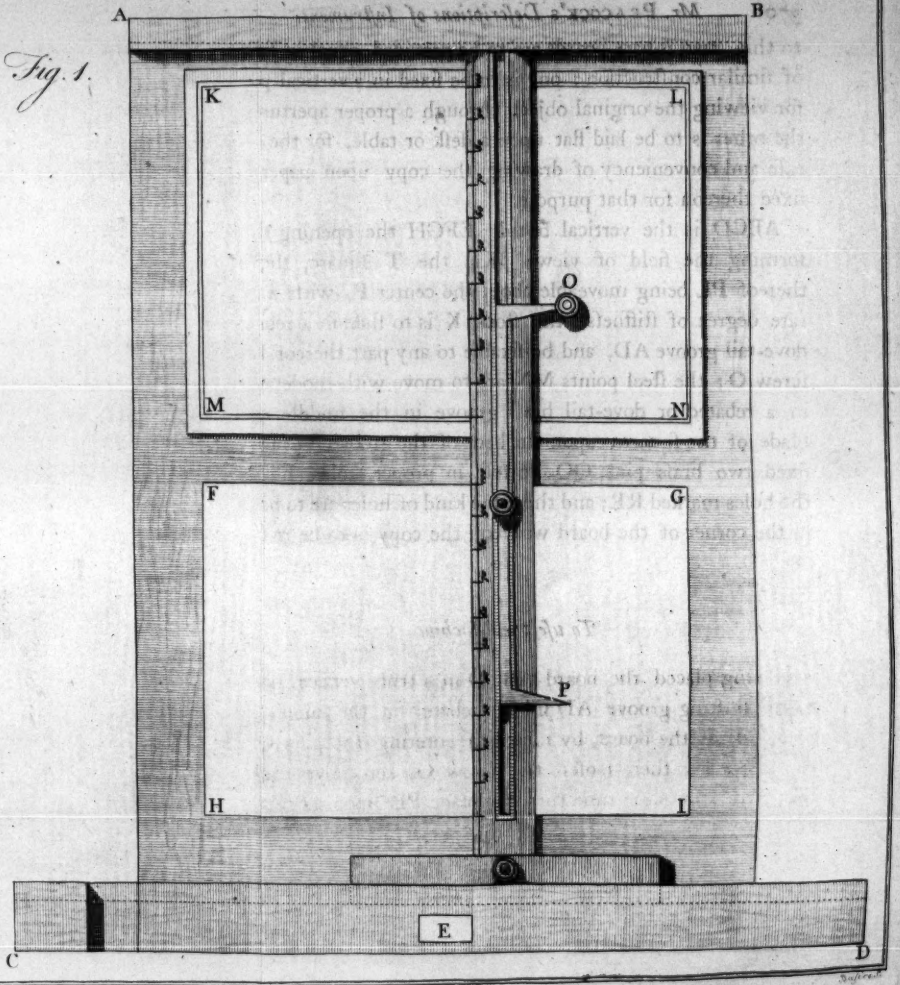


Fig. 2.

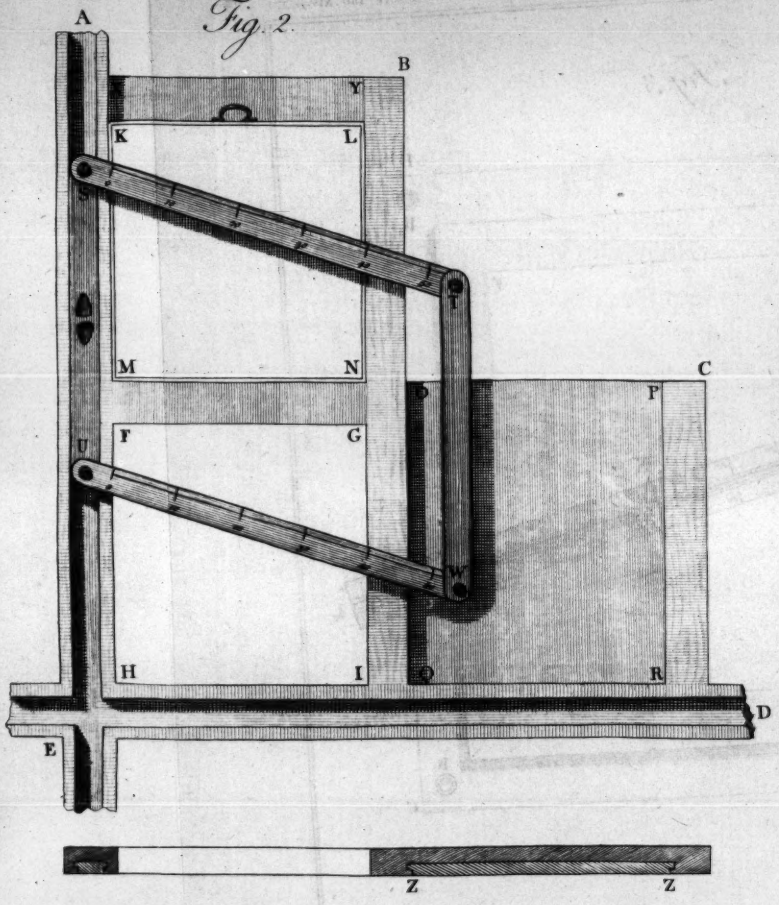
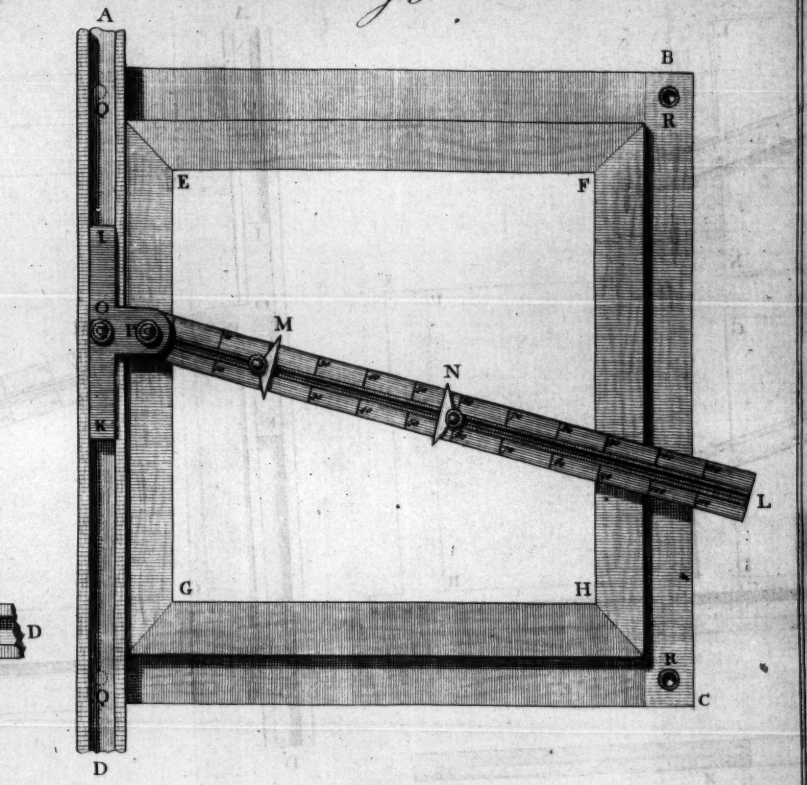


Fig. 3.



the correſponding ſide of the board on your deſk or table, and draw the line of its preciſe length and poſition.

N. B. If this is thought too operoſe, the braſs groove and ſliding pieces M, N, may be rejected, and the blade of the ſquare may be graduated on one or both of its edges at pleaſure; and all lines in the ſame direction may be drawn thereby exactly as to their poſitions, and nearly, though ſomewhat exceeding, their lengths *; and their preciſe lengths may be determined at the ſame time the lines in the contrary poſitions are drawn, whoſe lengths will be given at the ſame time by the lines firſt drawn.

* This will be effected, by noticing the numbers upon the blade, and taking thoſe next beyond the apparent limits of the line; and by this means the drawing will advance without the leaſt confuſion.

XXIII. *Experiments on Air.*

By Henry Cavendiſh, *Eſq. F.R.S. and A.S.*

Read June 2, 1785.

IN a Paper, printed in the laſt volume of the Philoſophical Tranſactions, in which I gave my reaſons for thinking that the diminution produced in atmoſpheric air by phlogiſtication is not owing to the generation of fixed air, I ſaid it ſeemed moſt likely, that the phlogiſtication of air by the electric ſpark was owing to the burning of ſome inflammable matter in the apparatus; and that the fixed air, ſuppoſed to be produced in that proceſs, was only ſeparated from that inflammable matter by the burning. At that time, having made no experiments on the ſubject myſelf, I was obliged to form my opinion from thoſe already publiſhed; but I now find, that though I was right in ſuppoſing the phlogiſtication of the air does not proceed from phlogiſton communicated to it by the electric ſpark, and that no part of the air is converted into fixed air; yet that the real cauſe of the diminution is very different from what I ſuſpected, and depends upon the converſion of phlogiſticated air into nitrous acid.

The apparatus uſed in making the experiments was as follows. The air through which the ſpark was intended to be paſſed, was confined in a glaſs tube M, bent to an angle, as in fig. 1. (tab. XV.) which, after being filled with quickſilver, was inverted into two glaſſes of the ſame fluid, as in the figure. The air to be

be tried was then introduced by means of a ſmall tube, ſuch as is uſed for thermometers, bent in the manner repreſented by ABC (fig. 2.) the bent end of which, after being previouſly filled with quickſilver, was introduced, as in the figure, under the glaſs DEF, inverted into water, and filled with the proper kind of air, the end C of the tube being kept ſtopped by the finger; then, on removing the finger from C, the quickſilver in the tube deſcended in the leg BC, and its place was ſupplied with air from the glaſs DEF. Having thus got the proper quantity of air into the tube ABC, it was held with the end C uppermoſt, and ſtopped with the finger; and the end A, made ſmaller for that purpoſe, being introduced into one end of the bent tube M, (fig. 1.) the air, on removing the finger from C, was forced into that tube by the preſſure of the quickſilver in the leg BC. By theſe means I was enabled to introduce the exact quantity I pleaſed of any kind of air into the tube M; and, by the ſame means, I could let up any quantity of ſoap-lees, or any other liquor which I wanted to be in contact with the air.

In one caſe, however, in which I wanted to introduce air into the tube many times in the ſame experiment, I uſed the apparatus repreſented in fig. 3. conſiſting of a tube AB of a ſmall bore, a ball C, and a tube DE of a larger bore. This apparatus was firſt filled with quickſilver; and then the ball C, and the tube AB, were filled with air, by introducing the end A under a glaſs inverted into water, which contained the proper kind of air, and drawing out the quickſilver from the leg ED by a ſyphon. After being thus furniſhed with air, the apparatus was weighed, and the end A introduced into one end of the tube M, and kept there during the experiment; the way of forcing air out of this apparatus into the tube being by

thruſting

thruſting down the tube ED a wooden cylinder of ſuch a ſize as almoſt to fill up the whole bore, and by occaſionally pouring quickſilver into the ſame tube, to ſupply the place of that puſhed into the ball C. After the experiment was finiſhed, the apparatus was weighed again, which ſhewed exactly how much air had been forced into the tube M during the whole experiment; it being equal in bulk to a quantity of quickſilver, whoſe weight was equal to the increaſe of weight of the apparatus.

The bore of the tube M uſed in moſt of the following experiments, was about one-tenth of an inch; and the length of the column of air, occupying the upper part of the tube, was in general from 1½ to ¾ of an inch.

It is ſcarcely neceſſary to inform any one uſed to electrical experiments, that in order to force an electrical ſpark through the tube, it was neceſſary, not to make a communication between the tube and the conductor, but to place an inſulated ball at ſuch a diſtance from the conductor as to receive a ſpark from it, and to make a communication between that ball and the quickſilver in one of the glaſſes, while the quickſilver in the other glaſs communicated with the ground.

I now proceed to the experiments.

When the electric ſpark was made to paſs through common air, included between ſhort columns of a ſolution of litmus, the ſolution acquired a red colour, and the air was diminiſhed, conformably to what was obſerved by Dr. PRIESTLEY.

When lime-water was uſed inſtead of the ſolution of litmus, and the ſpark was continued till the air could be no further diminiſhed, not the leaſt cloud could be perceived in the lime-water; but the air was reduced to two-thirds of its original bulk; which is a greater diminution than it could have ſuffered by mere phlogiſtication, as that is very little more than one-fifth of the whole.

The

The experiment was next repeated with ſome impure dephlogiſticated air. The air was very much diminiſhed, but without the leaſt cloud being produced in the lime-water. Neither was any cloud produced when fixed air was let up to it; but on the further addition of a little cauſtic volatile alkali, a brown ſediment was immediately perceived.

Hence we may conclude, that the lime-water was ſaturated by ſome acid formed during the operation; as in this caſe it is evident, that no earth could be precipitated by the fixed air alone, but that cauſtic volatile alkali, on being added, would abſorb the fixed air, and thus becoming mild, would immediately precipitate the earth; whereas, if the earth in the lime-water had not been ſaturated with an acid, it would have been precipitated by the fixed air. As to the brown colour of the ſediment, it moſt likely proceeded from ſome of the quickſilver having been diſſolved.

It muſt be obſerved, that if any fixed air, as well as acid, had been generated in theſe two experiments with the lime-water, a cloud muſt have been at firſt perceived in it, though that cloud would afterwards diſappear by the earth being re-diſſolved by the acid; for till the acid produced was ſufficient to diſſolve the whole of the earth, ſome of the remainder would be precipitated by the fixed air; ſo that we may ſafely conclude, that no fixed air was generated in the operation.

When the air is confined by ſoap-lees, the diminution proceeds rather faſter than when it is confined by lime-water; for which reaſon, as well as on account of their containing ſo much more alkaline matter in proportion to their bulk, ſoap-lees ſeemed better adapted for experiments deſigned to inveſtigate the nature of this acid, than lime-water. I accordingly made ſome experiments to determine what degree of purity the

air ſhould be of, in order to be diminiſhed moſt readily, and to the greateſt degree; and I found, that, when good dephlogiſticated air was uſed, the diminution was but ſmall; when perfectly phlogiſticated air was uſed, no ſenſible diminution took place; but when five parts of pure dephlogiſticated air were mixed with three parts of common air, almoſt the whole of the air was made to diſappear.

It muſt be conſidered, that common air conſiſts of one part of dephlogiſticated air, mixed with four of phlogiſticated; ſo that a mixture of five parts of pure dephlogiſticated air, and three of common air, is the ſame thing as a mixture of ſeven parts of dephlogiſticated air with three of phlogiſticated.

Having made theſe previous trials, I introduced into the tube a little ſoap-lees, and then let up ſome dephlogiſticated and common air, mixed in the above-mentioned proportions, which riſing to the top of the tube M, divided the ſoap-lees into its two legs. As faſt as the air was diminiſhed by the electric ſpark, I continued adding more of the ſame kind, till no further diminution took place: after which a little pure dephlogiſticated air, and after that a little common air, were added, in order to ſee whether the ceſſation of diminution was not owing to ſome imperfection in the proportion of the two kinds of air to each other; but without effect *. The ſoap-lees being then poured out of the tube, and ſeparated from the quick-

* From what follows it appears, that the reaſon why the air ceaſed to diminiſh was, that as the ſoap-lees were then become neutralized, no alkali remained to abſorb the acid formed by the operation, and in conſequence ſcarce any air was turned into acid. The ſpark, however, was not continued long enough after the apparent ceſſation of diminution, to determine with certainty, whether it was only that the diminution went on remarkably ſlower than before, or that it was almoſt come to a ſtand, and could not have been carried much further, though I had perſiſted in paſſing the ſparks.

ſilver,

ſilver, ſeemed to be perfectly neutralized, as they did not at all diſcolour paper tinged with the juice of blue flowers. Being evaporated to dryneſs, they left a ſmall quantity of ſalt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a ſolution of it, burned.

For more ſatisfaction, I tried this experiment over again on a larger ſcale. About five times the former quantity of ſoap-lees were now let up into a tube of a larger bore; and a mixture of dephlogiſticated and common air, in the ſame proportions as before, being introduced by the apparatus repreſented in fig. 3. the ſpark was continued till no more air could be made to diſappear. The liquor, when poured out of the tube, ſmelled evidently of phlogiſticated nitrous acid, and being evaporated to dryneſs, yielded 1 $\frac{4}{10}$ gr. of ſalt, which is pretty exactly equal in weight to the nitre which that quantity of ſoap-lees would have afforded if ſaturated with nitrous acid. This ſalt was found, by the manner in which paper dipped into a ſolution of it burned, to be true nitre. It appeared, by the teſt of *terra ponderoſa ſalita*, to contain not more vitriolic acid than the ſoap-lees themſelves contained, which was exceſſively little; and there is no reaſon to think that any other acid entered into it, except the nitrous.

A circumſtance, however, occurred, which at firſt ſeemed to ſhew, that this ſalt contained ſome marine acid; namely, an evident precipitation took place when a ſolution of ſilver was added to ſome of it diſſolved in water; though the ſoap-lees uſed in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an exceſs of alkali in it, ſome purified nitrous acid had been added to it, previous to the addition of the ſolution of ſilver. On conſideration, however, I ſuſpected, that this precipitation might ariſe from the nitrous acid in it being phlo-

gisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution, however, being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means.

Hence it is certain, that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think, that the precipitate, which our salt occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

In the above-mentioned Paper I said, that when nitre is detonated with charcoal, the acid is converted into phlogisticated air; that is, into a substance which, as far as I could perceive, possesses all the properties of the phlogisticated air of our atmosphere; from which I concluded, that phlogisticated air is

nothing elſe than nitrous acid united to phlogiſton. According to this concluſion, phlogiſticated air ought to be reduced to nitrous acid by being deprived of its phlogiſton. But as dephlogiſticated air is only water deprived of phlogiſton, it is plain, that adding dephlogiſticated air to a body, is equivalent to depriving it of phlogiſton, and adding water to it; and therefore, phlogiſticated air ought alſo to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with, dephlogiſticated air; only the acid formed this way will be more dilute, than if the phlogiſticated air was ſimply deprived of phlogiſton.

This being premiſed, we may ſafely conclude, that in the preſent experiments the phlogiſticated air was enabled, by means of the electrical ſpark, to unite to, or form a chemical combination with, the dephlogiſticated air, and was thereby reduced to nitrous acid, which united to the ſoap-lees, and formed a ſolution of nitre; for in theſe experiments thoſe two airs actually diſappeared, and nitrous acid was actually formed in their room; and as, moreover, it has juſt been ſhewn, from other circumſtances, that phlogiſticated air muſt form nitrous acid, when combined with dephlogiſticated air, the above-mentioned opinion ſeems to be ſufficiently eſtabliſhed. A further confirmation of it is, that, as far as I can perceive, no diminution of air is produced when the electric ſpark is paſſed either through pure dephlogiſticated air, or through perfectly phlogiſticated air; which indicates the neceſſity of a combination of theſe two airs to produce the acid. Moreover, it was found in the laſt experiment, that the quantity of nitre procured was the ſame that the ſoap-lees would have produced if ſaturated with nitrous acid; which ſhews, that the production of the nitre was not owing to any decompoſition of the ſoap-lees.

It

It may be worth remarking, that whereas in the detonation of nitre with inflammable ſubſtances, the acid unites to phlogiſton, and forms phlogiſticated air, in theſe experiments the reverſe of this proceſs was carried on; namely, the phlogiſticated air united to the dephlogiſticated air, which is equivalent to being deprived of its phlogiſton, and was reduced to nitrous acid.

In the above-mentioned Paper I alſo gave my reaſons for thinking, that the ſmall quantity of nitrous acid, produced by the exploſion of dephlogiſticated and inflammable air, proceeded from a portion of phlogiſticated air mixed with the dephlogiſticated, which I ſuppoſed was deprived of its phlogiſton, and turned into nitrous acid, by the action of the dephlogiſticated air on it, aſſiſted by the heat of the exploſion. This opinion, as muſt appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident, that dephlogiſticated air is able to deprive phlogiſticated air of its phlogiſton, and reduce it into acid, when aſſiſted by the electric ſpark; and therefore it is not extraordinary that it ſhould do ſo, when aſſiſted by the heat of the exploſion.

The ſoap-lees uſed in the foregoing experiments were made from ſalt of tartar, prepared without nitre; and were of ſuch a ſtrength as to yield one-tenth of their weight of nitre when ſaturated with nitrous acid. The dephlogiſticated air alſo was prepared without nitre, that uſed in the firſt experiment with the ſoap-lees being procured from the black powder formed by the agitation of quickſilver mixed with lead*, and that uſed

* This air was as pure as any that can be procured by moſt proceſſes. I propoſe giving an account of the experiment, in which it was prepared, in a future Paper.

in the latter from turbith mineral. In the firſt experiment, the quantity of ſoap-lees uſed was 35 meaſures, each of which was equal in bulk to one grain of quickſilver; and that of the air abſorbed was 416 ſuch meaſures of phlogiſticated air, and 914 of dephlogiſticated. In the ſecond experiment, 178 meaſures of ſoap-lees were uſed, and they abſorbed 1920 of phlogiſticated air, and 4860 of dephlogiſticated. It muſt be obſerved, however, that in both experiments ſome air remained in the tube uncondenſed, whoſe degree of purity I had no way of trying; ſo that the proportion of each ſpecies of air abſorbed is not known with much exactneſs.

As far as the experiments hitherto publiſhed extend, we ſcarcely know more of the nature of the phlogiſticated part of our atmoſphere, than that it is not diminiſhed by lime-water, cauſtic alkalies, or nitrous air; that it is unfit to ſupport fire, or maintain life in animals; and that its ſpecific gravity is not much leſs than that of common air: ſo that, though the nitrous acid, by being united to phlogiſton, is converted into air poſſeſſed of theſe properties, and conſequently, though it was reaſonable to ſuppoſe, that part at leaſt of the phlogiſticated air of the atmoſphere conſiſts of this acid united to phlogiſton, yet it might fairly be doubted whether the whole is of this kind, or whether there are not in reality many different ſubſtances confounded together by us under the name of phlogiſticated air. I therefore made an experiment to determine, whether the whole of a given portion of the phlogiſticated air of the atmoſphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the reſt, which would refuſe to undergo that change. The foregoing experiments indeed in ſome meaſure decided this point, as much the greateſt part of the air let up into the tube loſt its elaſticity; yet,

yet, as ſome remained unabſorbed, it did not appear for certain whether that was of the ſame nature as the reſt or not. For this purpoſe I diminiſhed a ſimilar mixture of dephlogiſticated and common air, in the ſame manner as before, till it was reduced to a ſmall part of its original bulk. I then, in order to decompound as much as I could of the phlogiſticated air which remained in the tube, added ſome dephlogiſticated air to it, and continued the ſpark till no further diminution took place. Having by theſe means condenſed as much as I could of the phlogiſticated air, I let up ſome ſolution of liver of ſulphur to abſorb the dephlogiſticated air; after which only a ſmall bubble of air remained unabſorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogiſticated air let up into the tube; ſo that if there is any part of the phlogiſticated air of our atmoſphere which differs from the reſt, and cannot be reduced to nitrous acid, we may ſafely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

The foregoing experiments ſhew, that the chief cauſe of the diminution which common air, or a mixture of common and dephlogiſticated air, ſuffers by the electric ſpark, is the converſion of the air into nitrous acid; but yet it ſeemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, ſome of this matter might be burnt by the ſpark, and thereby diminiſh the air, as I ſuppoſed in the above-mentioned Paper to be the caſe. The beſt way which occurred to me of diſcovering whether this happened or not, was to paſs the ſpark through dephlogiſticated air, included between different liquors: for then, if the diminution proceeded ſolely from the converſion of air into nitrous acid, it is plain that, when the dephlogiſticated air was perfectly pure, no diminution would take place; but when it contained

contained any phlogiſticated air, all this phlogiſticated air, joined to as much of the dephlogiſticated air as muſt unite to it in order to reduce it into acid, that is, two or three times its bulk, would diſappear, and no more; ſo that the whole diminution could not exceed three or four times the bulk of the phlogiſticated air: whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogiſticated air was, the greater and quicker would be the diminution.

The reſult of the experiments was, that when dephlogiſticated air, containing only $\frac{1}{120}$ of its bulk of phlogiſticated air (that being the pureſt air I then had), was confined between ſhort columns of ſoap-lees, and the ſpark paſſed through it till no further diminution could be perceived, the air loſt $\frac{43}{100}$ of its bulk; which is not a greater diminution than might very likely proceed from the firſt-mentioned cauſe; as the dephlogiſticated air might eaſily be mixed with a little common air while introducing into the tube.

When the ſame dephlogiſticated air was confined between columns of diſtilled water, the diminution was rather greater than before, and a white powder was formed on the ſurface of the quickſilver beneath; the reaſon of which, in all probability, was, that the acid produced in the operation corroded the quickſilver, and formed the white powder; and that the nitrous air, produced by that corroſion, united to the dephlogiſticated air, and cauſed a greater diminution than would otherwiſe have taken place.

When a ſolution of litmus was uſed, inſtead of diſtilled water, the ſolution ſoon acquired a red colour, which grew paler and paler as the ſpark was continued, till at laſt it became quite colourleſs and tranſparent. The air was diminiſhed

by almoſt half, and I believe might have been ſtill further diminiſhed, had the ſpark been continued. When lime-water was let up into the tube, a cloud was formed, and the air was further diminiſhed by about one-fifth. The remaining air was good dephlogiſticated air. In this experiment, therefore, the litmus was, if not burnt, at leaſt decompounded, ſo as to loſe entirely its purple colour, and to yield fixed air; ſo that, though ſoap-lees cannot be decompounded by this proceſs, yet the ſolution of litmus can, and ſo very likely might the ſolutions of many other combuſtible ſubſtances. But there is nothing, in any of theſe experiments, which favours the opinion of the air being at all diminiſhed by means of phlogiſton communicated to it by the electric ſpark.

Fig. 1

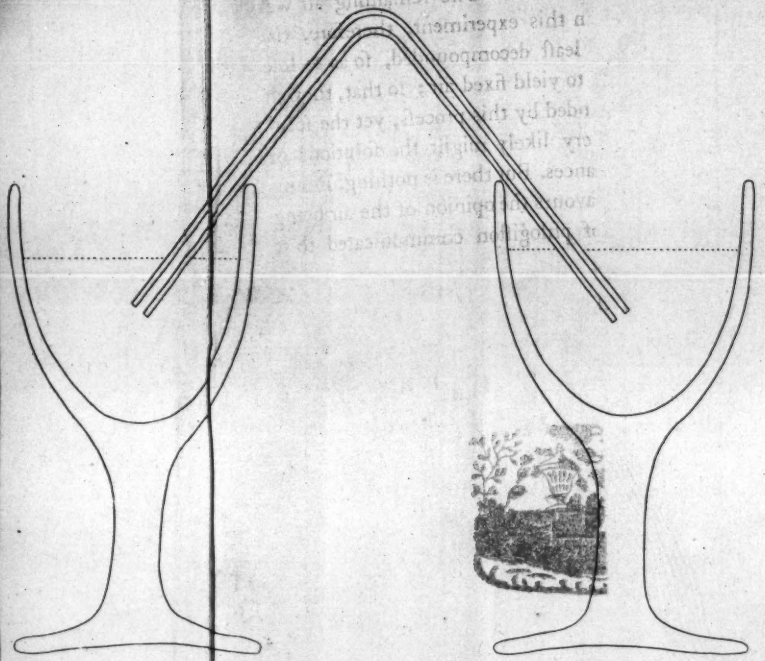


Fig. 2.

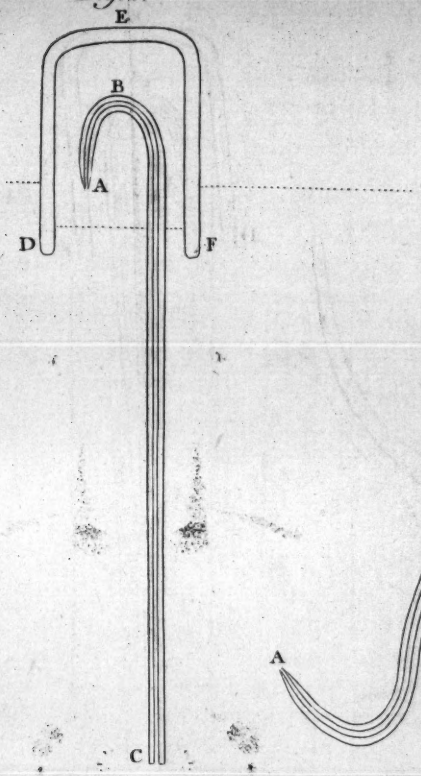
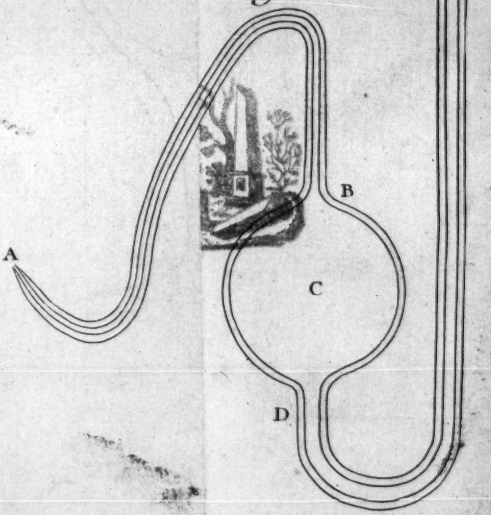


Fig. 3.



XXIV. *An Account of the Measurement of a Base on* Hounslow-Heath. *By Major-General* William Roy, *F.R.S. and A.S.*

Read from April 21 to June 16, 1785.

INTRODUCTION.

ACCURATE surveys of a country are universally admitted to be works of great public utility, as affording the surest foundation for almost every kind of internal improvement in time of peace, and the best means of forming judicious plans of defence against the invasions of an enemy in time of war, in which last circumstances their importance usually becomes the most apparent. Hence it happens, that if a country has not actually been surveyed, or is but little known, a state of warfare generally produces the first improvements in its geography: for in the various movements of armies in the field, especially if the theatre of war be extensive, each individual officer has repeated opportunities of contributing, according to his situation, more or less towards its perfection; and these observations being ultimately collected, a map is sent forth into the world, considerably improved indeed, but which, being still defective, points out the necessity of something more accurate being undertaken, when times and circumstances may favour the design.

The rise and progress of the rebellion which broke out in the Highlands of Scotland in 1745, and which was finally sup-

preſſed, by his Royal Highneſs the late Duke of Cumberland, at the battle of Culloden in the following year, convinced Government of what infinite importance it would be to the State, that a country, ſo very inacceſſible by nature, ſhould be thoroughly explored and laid open, by eſtabliſhing military poſts in its inmoſt receſſes, and carrying roads of communication to its remoteſt parts. With a view to the commencement of arrangements of this ſort, a body of infantry was encamped at Fort Auguſtus in 1747, under the command of the late Lord BLAKENEY, at that time a Major-General; at which camp my much reſpected friend, the late Lieutenant-General WATSON, then Deputy Quarter-Maſter-General in North Britain, was officially employed. This officer, being himſelf an engineer, active and indefatigable, a zealous promoter of every uſeful undertaking, and the warm and ſteady friend of the induſtrious, firſt conceived the idea of making a map of the Highlands. As aſſiſtant Quarter-Maſter, it fell to my lot to begin, and afterwards to have a conſiderable ſhare in, the execution of that map; which being undertaken under the auſpices of the Duke of CUMBERLAND, and meant at firſt to be confined to the Highlands only, was nevertheleſs at laſt extended to the Lowlands; and thus made general in what related to the mainland of Scotland, the iſlands (excepting ſome leſſer ones near the coaſt) not having been ſurveyed.

Although this work, which is ſtill in manuſcript, and in an unfiniſhed ſtate, poſſeſſes conſiderable merit, and perfectly anſwered the purpoſe for which it was originally intended; yet, having been carried on with inſtruments of the common, or even inferior kind, and the ſum annually allowed for it being inadequate to the execution of ſo great a deſign in the beſt manner, it is rather to be conſidered as a magnificent military

ſketch, than a very accurate map of a country. It would, however, have been completed, and many of its imperfections no doubt remedied; but the breaking out of the war of 1755 prevented both, by furniſhing ſervice of other kinds for thoſe who had been employed upon it.

On the concluſion of the peace of 1763, it came for the firſt time under the conſideration of Government, to make a general ſurvey of the whole iſland at the public coſt. Towards the execution of this work, whereof the direction was to have been committed to my charge, the map of Scotland was to have been made ſubſervient, by extending the great triangles quite to the northern extremity of the iſland, and filling them in from the original map. Thus that imperfect work would have been effectually completed, and the nation would have reaped the benefit of what had been already done, at a very moderate extra-expence.

It will not be expected, that I ſhould here attempt to aſſign cauſes for the long delay that has taken place in carrying a work of ſo laudable a nature into execution: ſuffice it to ſay, that a period of twelve years having elapſed, ſince the ſcheme had been firſt propoſed, as a work that could be beſt executed in time of profound peace, without any thing being done in it, previous to the nation's being unfortunately involved in the American war; it was ſufficiently obvious, that peace muſt be once more reſtored, before any new effort could be made for that purpoſe. In the mean while, as I ſtill entertained hopes that a work which ſeemed to merit the attention of the public, would, at ſome future period, be begun, and, by gradual perſeverance, ultimately brought to perfection; therefore, in the courſe of my ordinary military employments, wherein the very beſt opportunities have offered of acquiring a

thorough

thorough knowledge of the country, I have not failed to obſerve, at leaſt in a general way, ſuch ſituations as ſeemed to be the beſt adapted for the meaſurement of the baſes that would be neceſſary for the formation of the great triangles, and connecting the different ſerieſes of them together.

The peace of 1783 being concluded, and official buſineſs having detained me in or near town during the whole of that ſummer, I embraced the opportunity, for my own private amuſement, to meaſure a baſe of 7744.3 feet, acroſs the fields between the Jews-Harp, near Marybone, and Black-Lane, near Pancras; as a foundation for a ſeries of triangles, carried on at the ſame time, for determining the relative ſituations of the moſt remarkable ſteeples, and other places, in and about the Capital, with regard to each other, and the Royal Obſervatory at Greenwich. The principal object I had here in view (beſides that it might poſſibly ſerve as a hint to the public, for the revival of the now almoſt forgotten ſcheme of 1763) was, to facilitate the compariſon of the obſervations, made by the lovers of aſtronomy, within the limits of the projected ſurvey; namely, Richmond and Harrow, on the weſt; and Shooter's-Hill and Wanſted, on the eaſt: and thinking, that a Paper, containing the reſult of theſe trigonometrical operations, might not prove unacceptable to the Royal Society, I was engaged in making the computations for that purpoſe, when, very unexpectedly, I found, that an operation of the ſame nature, but much more important in its object, was really in agitation. This I ſaw would ſuperſede, at leaſt for the preſent, my own private obſervations, and perhaps render them wholly uſeleſs, unleſs it were as a matter of mere curioſity hereafter, to ſee how far ſuch as depended on a ſhort baſe, and a ſmall inſtrument (a quadrant of a foot radius) would agree with

those founded on a much longer base, and angles determined by a large circular instrument, being that proposed, as the best that could be made use of in the operation now to be mentioned.

In the beginning of October, 1783, Comte D'Adhemar, the French Ambassador, transmitted to Mr. Fox, then one of his Majesty's principal Secretaries of State, a Memoir of M. Cassini de Thury, in which he sets forth the great advantage that would accrue to astronomy, by carrying a series of triangles from the neighbourhood of London to Dover, there to be connected with those already executed in France, by which combined operations the relative situations of the two most famous observatories in Europe, Greenwich and Paris, would be more accurately ascertained than they are at present *.

This Memoir the Secretary of State, by his Majesty's command, transmitted to Sir Joseph Banks, the very respectable and worthy President of the Royal Society; who, about the middle of November, was pleased to communicate it to me, proposing at the same time, that I should, on the part of the Society, charge myself with the execution of the operation. To this proposition I readily assented, on being soon afterwards assured, through the proper official channels, that my undertaking it met with his Majesty's most gracious approbation.

A generous and beneficent Monarch, whose knowledge and love of the sciences are sufficiently evinced by the protection which he constantly affords them, and under whose auspices they are seen daily to flourish, soon supplied the funds that were judged necessary. What his Majesty has been pleased to

* M. Cassini's Memoir, with the Astronomer Royal's remarks on what is therein alledged, concerning the uncertainty of the relative situations of the two Observatories, will be given in the sequel.

give

give ſo liberally, it is our duty to manage with proper and becoming frugality, conſiſtent with the beſt poſſible execution of the buſineſs to be done, ſo as to make it redound to the credit of the Nation in general, and of this Society in particular.

The operation, whereof we are now to give ſome account, being the firſt of the kind, on any extenſive ſcale, ever undertaken in this country, naturally enough ſub-divides itſelf into two parts. Firſt, the choice and meaſurement of the baſe, with every poſſible care and attention, as the foundation of the work; ſecondly, the diſpoſition of the triangles, whereby the baſe is to be connected with ſuch parts of the coaſt of this iſland as are neareſt to the coaſt of France, and the determination of their angles, by means of the beſt inſtrument that can be obtained for the purpoſe, from which the reſult or concluſion will be drawn. It is the firſt part only, as a ſubject of itſelf ſufficiently diſtinct, that we are now to lay before the Society; it having been judged more adviſable, to ſhew that no time has been loſt in making reaſonable progreſs, than to defer the account till the whole operation ſhould be ultimately completed.

Choice of the Baſe. Tab. XVI.

Hounſlow-Heath having always appeared to be one of the moſt eligible ſituations, for any general purpoſe of the ſort now under conſideration, becauſe of its vicinity to the Capital and Royal Obſervatory at Greenwich, its great extent, and the extraordinary levelneſs of its ſurface, without any local obſtructions whatever to render the meaſurement difficult; being likewiſe commodiouſly ſituated for any future operations of a ſimilar nature, which his Majeſty may pleaſe to order to be

extended

extended from thence, in different directions, to the more remote parts of the island, it was proposed to Sir JOSEPH BANKS, that the local circumstances should be actually examined; so far, at least, as to enable us to form some judgement, of the best position of the line to be measured.

The 16th day of April, 1784, being accordingly fixed on for the purpose, and Mr. CAVENDISH and Dr. BLAGDEN accompanying the President on this occasion, we began our observations at a place called King's Arbour, at the north-west extremity of the Heath, between Cranford-Bridge and Longford; and having proceeded from thence through the narrow gorge, formed by Hanworth-Park and Hanworth-Farm, we finished at Hampton Poor-house, near the side of Bushy-Park, at the south-east extremity; the total distance, from the survey of Middlesex, being upwards of five miles.

On this inspection it was immediately perceived, that the first part of the operation, in order to facilitate the measurement, would be, the clearing from furze-bushes and ant-hills, a narrow tract along the heath, as soon as the ground should be sufficiently dry to permit the base to be accurately traced out thereon.

First tracing of the Base, and clearing of the Ground. Tab. XVI.

Chiefly with a view to the more effectual execution of the work, it was judged to be a right measure to obtain and employ soldiers, instead of country labourers, in tracing the base, clearing the ground, and assisting in the subsequent operations. For, at the same time that this was obviously the most frugal method, it was evident, that soldiers would be more attentive to orders than country labourers; and by encamping on the

ſpot would furniſh the neceſſary centinels, particularly during the night, for guarding ſuch parts of the apparatus, as it was foreſeen muſt remain carefully untouched, in the frequent interims of diſcontinuing and reſuming the work. Accordingly, a party of the 12th regiment of foot, conſiſting of a ſerjeant, corporal, and 10 men, were ordered to march from Windſor to Hounſlow-Heath, where they encamped on the 26th of May, cloſe by Hanworth Summer-houſe, to which ſpot the neceſſary tents, camp equipage, and entrenching tools, &c. had been previouſly ſent.

Whatever might have been the particular direction given to the baſe conſidered by its extremities, from conſulting the plan it will eaſily appear, that it muſt always neceſſarily lead through the narrow gorge of the Heath formed by Hanworth-Park and Hanworth-Farm. The firſt point therefore to be attended to, in tracing it out, was, that it might lead through this paſs, without interfering with certain ponds, or gravel-pits full of water, which are in it. Theſe were eaſily avoided by carrying the line pretty near to Hanworth Summer-houſe; and in directing the teleſcope from thence towards the ſouth-eaſt, it was accidentally found, that by leaving Hampton Poor-houſe a very little to the weſtward, or right, the line would coincide with a remarkable high ſpire, ſeen at the diſtance of eleven or twelve miles, and known afterwards to be Banſted-Church. As there could not be a better ſituated, or more conſpicuous object than this, therefore the firſt or ſouth-eaſt ſection of the baſe, comprehended between the Summer-houſe and the angle of the ſmall field adjoining to Hampton Poor-houſe, was immediately directed upon it; and the ſoldiers were the ſame day ſet to work to clear the tract, which, at a medium, was made from two to three yards in breadth. This

operation

operation continued eight or ten days, owing to the lower part of the heath, between Wolſey-River and the Poor-houſe, being encumbered with bruſh-wood.

When the clearing of the firſt ſection was completed, the ſecond, comprehended between the Summer-houſe and the great road leading from Staines to London, was traced out in the following manner. One of the pyramidal bell-tents (whereof two had been provided, one of twenty-five, and the other of fifteen feet in height) being placed at the ſtation near the Summer-houſe, camp colours were then arranged from diſtance to diſtance, ſo as to be in a line with the bell-tent and Banſted ſpire. In like manner, the third ſection, comprehended between the Staines Road and King's Arbour, was traced out.

This firſt tracing of the baſe was done by means of a common teleſcope held in the hand only, that no time might be loſt in employing the ſoldiers to ſmooth the tract which was to be meaſured; becauſe the tranſit inſtrument (my own property, for which a portable ſtand had been for ſome time preparing) was not yet ready to be applied, as it afterwards was, in tracing out the baſe more accurately.

The camp ſtill remained, where it was originally pitched, at the angle of Hanworth-Park, this being a very convenient poſition, with regard to the firſt and ſecond ſections; but being too remote from the third, that time might not be loſt, and the men unneceſſarily fatigued in marching backwards and forwards; therefore, one half of the party, under the command of the corporal, was detached to the northward, and quartered in the neighbouring villages, to clear the third ſection, while the ſerjeant, with the remainder, were occupied in ſmoothing the ſecond. Owing to the extraordinary wetneſs of the ſeaſon, this operation required more time than had been at firſt

imagined, not having been entirely finiſhed before the firſt week of July. We ſhall therefore leave it going on, and in the mean time proceed to deſcribe the inſtruments that were ſubſequently made uſe of in the firſt and ſecond meaſurements.

Steel Chain. Tab. XVII.

One of the firſt inſtruments, which that able artiſt Mr. RAMSDEN had orders to prepare, was a ſteel chain, one hundred feet in length, the beſt that he could make. Not that it was intended, nor could it be ſuppoſed, that we ſhould abſolutely abide by the reſult that this chain ſhould furniſh us with, for the length of the baſe; but it was hoped, that an inſtrument of this ſort might be made, which would meaſure diſtances much more accurately than any thing of that kind had ever done before: and it was conſidered as an object of ſome conſequence, to endeavour to ſimplify, and render as eaſy as poſſible, the meaſurement of baſes in future: an operation which, hitherto, has always been found to be tedious and troubleſome, to which we may now further add, uncertain likewiſe, when done with rods of deal, as will appear from the account hereafter to be given.

The conſtruction of the chain, which is on the principles of that of a watch, will be underſtood from the repreſentation of ſome of its chief parts, to the full ſize, in tab. XVII. where the firſt, or zero-end link, is ſhewn both in plan and elevation, in the ſtate in which it was originally applied to meaſurement on the ſurface of the ground. Each link conſiſts of three principal parts, namely, a long plate; two ſhort ones, half the thickneſs of the former, with circular holes near the

extremities of each; and two caſt-ſteel pins, or axes, ſuited to the diameters of the holes, which ſerve to connect the adjoining links together. The holes in the ſhort plates are made rough or jagged with a file; ſo that when they have embraced the ends of two adjoining long ones, and the pins have paſſed through all the holes, in rivetting their extremities, they are made perfectly faſt, and as it were united to the ſhort plates; while the embraced ends of the long ones turn freely round on the middle part of the pins.

At every tenth link the joint, juſt now deſcribed, has a poſition at right-angles to the former; that is to ſay, the ſhort plates lie here horizontally, and the pins paſſing through them ſtand vertically. Thus, there being in the whole chain two hundred caſt-ſteel pins, one hundred and eighty lie horizontally; and twenty, including the two by which the handles are attached, ſtand vertically. Theſe croſs-joints, which were chiefly intended that the chain might fold up in a ſmaller compaſs, by returning upon itſelf at every tenth link, are likewiſe uſeful in preſenting a horizontal ſurface, to which ſmall circular pieces of braſs are ſcrewed, with figures 1, 2, 3, &c. to 9, engraved on them, denoting the decimal parts of the length. Thus the middle croſs-joint, or that which ſeparates the 50th from the 51ſt link, is ſhewn in the Plate with the figure 5 upon it.

The chain, in its firſt conſtruction (for we are now to point out ſome alterations that were afterwards made in it), was one hundred feet in length, including the two braſs handles; in the extremity of each of which there was a ſemi-circular hole, of the ſame diameter with the ſteel arrows ſucceſſively fixed in the ground, and ſerving to keep the account of the number of chains, when applied to common meaſurement. In this its firſt

firſt mode of application it was ſoon diſcovered, as we ſhall have occaſion to mention hereafter, how admirably the chain performed; and that, with ſome farther precautions, a ſtill greater degree of exactneſs might be attained, by ſupporting it on ſtands, or even on planks, laid on, or but little removed from, the common ſurface of the earth. For this purpoſe, the two end-links were altered, each being now made equal to one foot, excluſive of the handles. By referring to tab. XVII. the nature of this alteration will be eaſily conceived. It conſiſted in ſcrewing to the under ſide of the handles, very near the joints, two feather-edged pieces of braſs *; the one denoting zero, and the other 100 feet. Over the dart at the firſt, a plummet with a fine ſilver wire being ſuſpended, that wire, by a very ſimple apparatus, hereafter to be deſcribed, may be brought accurately to coincide with any point whatever of commencement: and at the ſecond, a fine line with a knife, or other ſharp inſtrument, being drawn on a piece of card placed there for the purpoſe, and changed as often as needful; or, as was likewiſe practiſed, and found to anſwer better, a line on a moveable ſlide of braſs, attached to the top of the ſtand or plank, being brought to coincide with the feather-edge, and then faſtened underneath; the extremity of the 100 feet is readily aſcertained: and thus the meaſurement may be continued on with great accuracy to any diſtance at pleaſure.

That the chain, in this its altered ſtate, may ſtill be advantageouſly applied to ordinary meaſurement on the ſurface of the earth, the pieces above deſcribed, having ſteady pins, and being faſtened with ſcrews, can be eaſily removed, and others,

* They were originally of braſs, but are now of ſteel, that the edges by being harder might run leſs riſk of being damaged.

exactly of the ſame length, ſubſtituted in their ſtead, with ſemi-circular holes (as repreſented in the Plate by dotted lines near the joint of the handle) to receive the ſteel arrows, then to be made uſe of in the manner already mentioned.

This moſt excellent chain ſeems not to have ſuffered any perceptible extenſion from the uſe that has hitherto been made of it. It is ſo accurately conſtructed, that when ſtretched out on the ground, as in common uſe, all the long plates lying vertically or edge-wiſe, if a perſon, laying hold of either end with both hands, gives it a flip or jerk, the motion is, in a few ſeconds, communicated to the other end, in a beautiful vertical ſerpentine line; when the perſon, holding that handle, receives a ſudden ſhock, by the weight of the chain pulling him forcibly. The chain weighs about eighteen pounds, and when folded up is eaſily contained in a deal box, about fourteen inches long, eight inches broad, and the ſame in depth.

Deal Rods. Tab. XVIII.

The baſes which have hitherto been meaſured in different countries, with the greateſt appearance of care and exactneſs, have all, or for the moſt part, been done with deal rods of one kind or other, whoſe lengths being originally aſcertained by means of ſome metal ſtandard, were, in the ſubſequent applications of them, corrected by the ſame ſtandard. Having thus had ſo many precedents, ſerving as examples to guide us in our choice, it was natural enough that we ſhould purſue the ſame method in the meaſurement to be executed on Hounſlow-Heath; taking, however, all imaginable care, that our rods ſhould be made of the very beſt materials that could be procured;

cured; with this farther precaution, that by truſſing them, they ſhould be rendered perfectly inflexible, a circumſtance not before attended to.

As ſome difficulty had been found in procuring well-ſeaſoned Pine-wood of ſufficient length, and perfectly free from knots, for the intended purpoſe; therefore Sir Joseph Banks had early applied to the Admiralty for aſſiſtance in this reſpect; and forthwith obtained an order to be furniſhed with what we might have occaſion for, from his Majeſty's yard at Deptford, where an old New-England maſt, and alſo one of Riga wood, were ſpeedily cut up for our uſe.

New-England white Pine is lighter, leſs liable to warp, and leſs affected by moiſture, than Riga red wood. But the New-England maſt, when it came to be very minutely examined, was found to be too much wounded by ſhot-holes in ſome parts, or too much decayed or knotty in others, to afford us a ſufficiency. This being the caſe we had recourſe to the Riga wood, which was indeed extremely ſmooth and beautiful; and ſo perfectly ſtraight-grained, that a fibre of it, when lifted up, might be drawn, like a thread, almoſt from one end to the other.

It had been in contemplation, to make the rods of twenty-five or thirty feet in length; and one of the former dimenſions was actually conſtructed: but this being found to be rather too unwieldy, it was judged beſt to content ourſelves with thoſe of about twenty feet.

Different opinions have been entertained with regard to the beſt mode of applying rods in meaſurement; ſome contending that contacts, or that of butting the end of one rod againſt the end of the other, is the beſt; while others (with more probability of being

being right) are of opinion, that the adjuſtment by the coincidences of lines ſhould have the preference. The firſt is undoubtedly the moſt expeditious method; but ſeems at the ſame time to be liable to this very objectionable circumſtance, that the probable errors fall all one way: whereas, in the ſecond method, although by far the moſt tedious, the errors of coincidence falling ſometimes on one ſide, and ſometimes on the other, they compenſate for, or deſtroy, each other; and therefore no error is committed.

With the view of ſatisfying both parties, and in order to put the matter, if poſſible, out of doubt, it was judged proper to conſtruct the rods in ſuch a manner as to admit of both methods being tried, that we might adhere to that which ſhould be found by experience to be the beſt.

Three meaſuring rods were accordingly ordered to be made, and alſo a ſtandard rod, with which the former were from time to time to be compared. Their general conſtruction will be better conceived from the plan and elevation, and other repreſentations of their principal parts, in tab. XVIII. than by any deſcription, however particular, conveyed in words. It will be ſufficient to ſay, that the ſtems of the three meaſuring rods are each twenty feet three inches in length, reckoning from the extremities of the bell-metal tippings; very near two inches deep, and about 1¼ inch broad. Being truſſed laterally and vertically, they are thereby rendered perfectly, or at leaſt as to ſenſe, inflexible. The ſtandard rod could only be truſſed laterally; and it is juſtly repreſented by the plan of the other rods, excepting that its ſtem is ſomething ſtronger, and that it has two or three inches at each end of extra-length, the reaſons for which differences will appear hereafter.

By referring to the Plate it will be obſerved, that two narrow pieces of ivory, each faſtened with two ſmall ſcrews, are inlaid into the upper ſurface of the rods, within one inch and a half of the extremities of the tippings. Theſe ivory pieces received the fine black lines cut into them when the lengths of the rods were laid off, in the manner hereafter to be mentioned, and accurately determined the intermediate diſtance of 20 feet, or 240 inches, the meaſure to be uſed in the application by coincidences: whereas, in that by contacts, the ſpace comprehended between the extremities of the projecting lips of the tippings, is 243 inches.

Immediately behind each ivory piece, a cavity is formed underneath, in the middle of the ſtem. This receives a braſs wheel, about eight-tenths of an inch in diameter, whoſe axis turns in the fork of a braſs ſpring, five inches long, faſtened by a ſcrew to the under ſurface juſt before the croſs feet. Theſe ſprings are only of ſuch ſtrength as to permit the wheels to be forced up into the cavities by the weight of the rod, which, in its adjuſted ſtate, always reſts entirely on the ſurfaces of the two ſtands that ſupport its extremities. But when the rod is to be raiſed from the ſtands, then the milled-headed ſcrews, projecting above the upper ſurface, and ſtanding over the middle of the ſprings, being brought to act, the wheels are thereby preſſed downwards, and receive the full weight of the rod, which is then eaſily moved backwards or forwards to its true poſition, either of contact or coincidence.

The croſs-feet, placed about 5¼ inches from the ends of the rods, and ¼ inch from the inſertion of the truſſings, are each about nine inches long, 1¼ broad, and nearly an inch in depth, having their lower ſurfaces level with that of the ſtem. By

means of theſe, the rods are not only kept more ſteady on the ſtands, againſt the common action of the wind upon the truſſings; but they likewiſe ſerve as holds for the vertical and horizontal braſs clamps, whereby the rods are made faſt to the ſtands on one ſide or other, and in both modes of application, contacts and coincidences; as will be more fully explained hereafter, in deſcribing the tops of the ſtands.

Braſs Standard Scale, and method of laying off the lengths of the Deal Rods.

At the ſale of the inſtruments of the late ingenious optician Mr. JAMES SHORT, I purchaſed a finely divided braſs ſcale, of the length of 42 inches, with a VERNIER's diviſion of 100 at one end, and one of 50 at the other, whereby the 10000th part of an inch is very perceptible. It was originally the property of the late Mr. GRAHAM, the celebrated Watch-maker; has the name of JONATHAN SISSON engraved upon it; but is known to have been divided by the late Mr. BIRD, who then worked with SISSON.

It is ſufficiently well known to this Society, that their braſs ſtandard ſcale, about 42 inches long, which contains on it the length of the ſtandard yard from the Tower, that from the Exchequer, and alſo the French half-toiſe, together with the duplicate of the ſaid ſcale, ſent to Paris for the uſe of the Royal Academy of Sciences, were both made by Mr. JONATHAN SISSON, under Mr. GRAHAM's immediate direction. Now, although there ſeemed to be every reaſon to ſuppoſe, that the ſcale at preſent in my poſſeſſion, originally Mr. GRA-

HAM's property, would correſpond with thoſe above-mentioned, which he had been directed by the Royal Society, with ſo much care and pains, to provide; yet, that nothing of this ſort might remain doubtful, it was judged right, in ſettling the abſolute length of the baſe, which I meaſured near London in 1783, as has been mentioned in the introduction to this Paper, that the two ſcales ſhould be actually compared. Having accordingly obtained an order from the Preſident, for admiſſion into the Society's Apartments, I went there in the afternoon of the 13th of Auguſt, and laid both ſcales taken out of their caſes on the table of the meeting-room, with thermometers along-ſide of them, that they might acquire the ſame temperature. On the forenoon of the 15th of Auguſt the compariſon was made, with the aſſiſtance of Mr. RAMSDEN, who for that purpoſe carried along with him his curious beam-compaſſes, whoſe micrometer-ſcrew ſhews very perceptibly a motion of $\frac{1}{1000}$th part of an inch. Thus the extent of three feet, being carefully taken from the Society's ſtandard, and applied to my ſcale, it was found to reach exactly to 36 inches, the temperature being 65°. In like manner, the beam-compaſſes being applied to the length of the Exchequer yard, the extent was now found by the micrometer to overreach that yard by $\frac{6}{10000}$th, or nearly $\frac{1}{1000}$th parts of an inch.

Having thus ſhewn that my ſcale is accurately of the ſame length with the Society's ſtandard, it remains to point out the uſe that was made of it, for aſcertaining the lengths of the deal rods, intended for the operation on Hounſlow-Heath. In the firſt place, Mr. RAMSDEN prepared a beam-compaſs, ſufficient to take in twenty feet, truſſed in all reſpects like the

mea-

measuring rods, but something deeper, and fitted as usual with proper points and micrometer. The standard rod being now constructed was laid on the shop-board, strongly framed for the purpose, and nearly level. To one side of it, at the distance of about twenty feet two inches from center to center, two strong bell-metal cocks were firmly screwed. These cocks were about 2¼ inches in length, three-eighths in thickness, and rose above the stem nearly two inches, so as to be on the same plane with the surface of the measuring rods, when placed upon it.

A large plank, cut from the New-England mast, upwards of thirty feet long, nine or ten inches broad, and about three inches thick, being set edge-wise in the same room, on part of the stands now ready for the operation, was, in that position, planed perfectly smooth and straight. A silver wire being then stretched very tight, along the middle of the plank, from one end to the other, six spaces of forty inches each were marked off by the side of the wire, at which points seven brass pins, about one-tenth of an inch in diameter, were driven into the wood, and their tops polished with the stone. During the whole of this operation, and that which followed, the thermometer, lying by the side of the brass scale, continued steadily at or very near 63°.

A fine dot being now made on one of the extreme pins, and the silver wire being stretched over the dot, and as near as possible over the middle of the other pins, in which position it was made fast; the extent of forty inches, taken with the utmost care from the brass scale, was then marked off, by placing one point of the beam-compasses in the dot, and with the other describing a short faint arc on the surface of the se-

cond

cond pin. The beam being then removed, and one point placed in the interſection of the arc and wire, with the other point a dot was made on the third pin, under the middle of the wire. Upon this dot, as a center, a faint arc was next deſcribed on the ſame pin where the firſt had been traced. In this manner the ſix times forty inches were marked off, alternately with dots and arcs; a method found by Mr. RAMSDEN, in his practice, to be more accurate, than when dots only are made uſe of.

The exact length of twenty feet, thus obtained, was next taken between the points of the long beam-compaſſes, and transferred to the tops of the bell-metal cocks, placed, as has been already mentioned, on the ſide of the ſtandard rod, in ſuch manner as to leave more than one inch and a half of the ſaid cocks beyond or without the lines denoting the extent of the twenty feet. This being done, the meaſuring rods were ſucceſſively placed on the ſtandard, and their ſides applying cloſe to the cocks, the diſtance of twenty feet was readily transferred from them to the inlaid ivory pieces, on which fine lines were afterwards cut, by marks accurately made for that purpoſe.

With regard to the adjuſtment of the lips of the bell-metal tippings, which extend exactly one inch and a half beyond the ivory lines, ſo as to make the total length of the rod 243 inches, it is to be obſerved, that they terminate in flat curves of 36 inches radius, paſſing through the inch and half points, to which they were cautiouſly ground down, that at firſt they might rather exceed than be defective in length. Any two of the rods, lying in the ſame plane, and alſo in the ſame ſtraight line, being brought into contact with each other; if

of

of the true length, the ſpace in that poſition, comprehended between the two lines on the inlaid ivory pieces, muſt be exactly three inches. For the purpoſe of this adjuſtment, the extent of three inches was therefore taken from the braſs ſcale and cut upon the ſide of a detached piece of ivory; which being readily applied to the aforeſaid intermediate ſpace, the ſame was gradually reduced, by grinding the lips equally, till it exactly correſponded with that taken from the ſcale.

The three rods are numbered by a cypher on the ſurface of the metal at each end, 1.2; 3.4; 5.6; and that being the order in which they were to be applied in actual meaſurement, ſo it was likewiſe the order in which they were adjuſted; that is to ſay, the rod 1.2 was adjuſted with 3.4, and with 5.6; and the rod 3.4 was, in like manner, adjuſted with 1.2 and 5.6.

One of theſe deal rods, when finiſhed, was found to weigh twenty-four pounds. They were intended to be contained in two cheſts, one large and the other ſmaller. The large cheſt, which is about 2¼ feet deep, may be called a double one, becauſe it has two lids that lift quite off, which, in turning upſide down, become alternately top and bottom, having between them, but much nearer to the one than the other, a bottom that is common to both. The ſhallow ſide holds the ſtandard rod; and the other, two of the meaſuring rods; which laſt is rendered practicable by having one of the ſide braces of each fixed only with ſcrews, ſo as to be removed and replaced at pleaſure. Thus one of the rods being laid in its place, the other is put over it in an inverted poſition; and both having the proper faſtenings to keep them in their poſitions, the lid is then put on, and fixed by ſcrews. The cheſt being now turned upſide down, and the other lid removed, the ſtandard is thereby

thereby diſcovered reſting on the common bottom, which has bands laid acroſs it for the purpoſe, a few inches below what has now become the ſurface of the cheſt. It was neceſſary that the ſtandard ſhould reſt thus high, both that the light might come freely upon it, and that, being ſupported by the deep ſides of the cheſt, it might be prevented from twiſting, for it will be remembered that it is only truſſed laterally. By means of a ſmall braſs ſpring fixed to each end of the ſtandard, a fine ſilk thread, as being leſs liable to accident than ſilver wire, is ſtretched along its ſtem, which by ſmall wedges prepared for the purpoſe, and ſlipped in between it and the bands on which it reſts, is always brought into the ſame poſition. This being done, the ſilk thread is turned off, ſo as to permit the meaſuring rods to be laid on the ſtandard for compariſon. With regard to the ſmaller cheſt, ſuch a one was actually made, and ſent down to the heath, towards the cloſe of the operation with the deal rods; but from ſome miſtake in its dimenſions, it would not admit the third rod.

Stands for the Meaſuring Rods. Tab. XVIII. and XIX.

From the extraordinary levelneſs of Hounſlow-Heath, the aſcent from the ſouth-eaſt towards the north-weſt being little more than one foot in a thouſand in the diſtance of five miles, it was eaſily ſeen, that the computed baſe-line, or that actually forming a curve parallel to the ſurface of the ſea, at that height above it, would fall ſo little ſhort of the hypothenuſal diſtance, meaſured on, or parallel to, the ſurface of the Heath, as ſcarcely to deſerve notice, had it not been thought neceſſary to

ſhew, how much one end of the baſe was really higher than the other; and to convince the world, that in an operation of this ſort, where ſo much accuracy was expected, no pains were ſpared, nor the moſt trivial circumſtances neglected.

From the trouble and uncertainty attending the frequent uſe of plummets, eſpecially in windy weather, inſtead of meaſuring level or baſe lines, as has hitherto been cuſtomary (in which caſe it would have been neceſſary to make uſe of the plummet, or ſome ſuch contrivance, at every ſtep of aſcent or deſcent) it was judged to be a better method to meaſure hypothenuſes, and, having obtained the relative heights of the ſtations by the accurate application of the teleſcopic ſpirit-level, to compute the baſe lines. Thus it was propoſed, that the length of the baſe on Hounſlow-Heath ſhould be obtained by meaſuring a line through the air, drawn parallel to the common ſurface from ſtation to ſtation, in equal diſtances of 200 yards or 600 feet each, as repreſented in the figure at the top of tab. XVIII.

For this purpoſe, two kinds of ſtands were uſed; one whoſe height was fixed, to be placed at the beginning and end of each 200 yards; and the others, whoſe heights were moveable, that their ſurfaces might be brought more eaſily to coincide with the line paſſing through the air from one fixed ſtand to the other. The fixed ſtands in their firſt ſtate, repreſented by that towards the left-hand in the plate for the deal rods, were only two feet ſeven inches in height; but when the glaſs rods were afterwards uſed, they had an additional piece of ten inches faſtened to the top (as in the left-hand ſtand of tab. XIX.) which made their total height above the Heath, including the platform on which they ſtood, three feet and a half. They are tripods of white deal, whoſe legs extend about

three feet from each other; and being braced diagonally, are mortoiſed at top into circles of the ſame ſort of wood. Over this circle, a ſquare table of about 11¼ inches is fixed, compoſed of oak, and mahogany at top; but both taken together do not exceed 1¼ inch in thickneſs.

The nature of the moveable ſtands, whereof there were at laſt no fewer than ſeventeen provided, will be comprehended from the repreſentations of them towards the right-hand in tab. XVIII. and XIX. Their general conſtruction, in what regards the part of them which is fixed, differs not from that of the others, excepting that they were of different heights, from two feet to about two feet eight inches, ſo as better to ſuit the irregularities of the ground where it might be neceſſary to place them. In the middle of each of theſe, an hexagonal wooden pipe deſcends, from the top to within two or three inches of the bottom, where it is joined by a brace reaching from each leg. This pipe receives the common cheeſe preſs wooden ſcrew (having three ſides ſcrewed and three plane), to the top of which the ſquare table is attached. It is embraced by the circular nut, or winch with four handles, whereby the table is elevated or depreſſed at pleaſure; and being brought to its proper height, is there made perfectly faſt by means of the flat-headed iron ſcrew, which paſſing through one of the legs, preſſes an iron plate, fixed in the inſide of the pipe, againſt one of the plane ſides of the ſcrew.

In deſcribing the deal rods, there has already been occaſion to make mention of the vertical and horizontal clamps, whereby the croſs-feet are faſtened to the table on the top of the ſtand. The nature of theſe tables will be beſt underſtood by conſulting the two plans of them towards the right hand in tab. XVIII.; whereof one repreſents the two grooves fitted for the alternate reception

of the horizontal clamp, according to the ſide on which the rod lies that is to be moved on into coincidence; and the other ſhews it actually in its place, with the clamp itſelf detached in elevation along-ſide of it. Thus from the plan it may be perceived, that the firſt, or adjuſted rod, lies towards the farther ſide of the table, and is there ſecured by the vertical clamp. The ſecond, or moveable rod, lies on the hither ſide, and therefore the horizontal clamp is placed in the farther groove, where it is firmly pinched by the nut underneath. The rod has been brought to coincidence by working with the two milled-headed ſcrews againſt the oppoſite ſides of the croſs-foot. This apparatus, although perfectly good in theory, was found to be much too confined in its nature to anſwer well in practice, requiring the ſtands to be placed with a degree of preciſion, which could not be effected in the field without great loſs of time; and this was the real cauſe, as will be ſeen hereafter, that the meaſurement by coincidences with the deal rods was given up, and that by contacts adhered to.

Towards the left-hand of tab. XVIII. the plan of one of the ſquare tables is repreſented with the ends of the ſecond and third rods upon it in contact. In this operation it will be perceived, that only one croſs-foot of each rod could now reſt on and be clamped to the ſtand, the tables having been inadvertently cut too ſmall to admit of both; and although this has the appearance of imperfection, yet no inconvenience whatever was found to reſult from it in practice, experience having ſhewn, that the clamping of either end ſufficed to keep the rod ſteady. Along-ſide of the table, the vertical clamp, being that now ſolely made uſe of, is likewiſe repreſented in elevation.

On the face or exterior ſide of each leg of all the ſtands, fixed as well as moveable, a plate of braſs is ſcrewed near the bottom, with two holes in each, over a groove purpoſely made in the wood underneath. By means of theſe plates, parallelopid leaden weights, about fourteen pounds each, having braſs pins with heads ſuited to enter the holes, and fall down in the grooves, into a narrow-pointed part of them, are readily ſlipped on or off each leg. Thus every ſtand, excluſive of its own weight, which is about thirty-one pounds, being loaded with forty-two pounds of lead, is thereby rendered perfectly firm and ſteady.

A number of wedges were alſo prepared, and always ready to be placed under the legs; by means of which, and a ſpirit level laid on the table, its plane is brought to the proper poſition.

Notwithſtanding all theſe precautions, it having been found, in the meaſurement with the deal rods, that time was loſt in levelling the ſtands, particularly in ſituations where the ſurface happened to be more than uſually uneven, or where it was of a looſe or ſpongy nature; therefore Mr. Smeaton adviſed (and no man's advice is more deſerving of attention), that deal platforms, ſtanding on pickets driven into the ground, and properly levelled, ſhould be uſed to receive the legs of the ſtands. Accordingly, for the operation with the glaſs rods (table XIX.) twenty ſuch triangular platforms made of inch deal, whoſe ſides were each three feet two inches in length, and void in the middle, were provided; as alſo a number of beech-pickets, about an inch and a half ſquare, and of different lengths, from ſeven to twelve or fourteen inches. Three of theſe pickets, ſhort or long as the ſituation required, being driven into the ground, till their heads (by the carpenter's

level) were brought to the proper height, the platform was laid upon them; and on that the ſtand itſelf being placed, its poſition was ultimately corrected by the ſpirit level laid on the top of the table. Each of the beech pickets had a hole bored through its top, fit to receive a piece of ſtrong tent-line, by which, and the help of one of the camp mallets, the pickets were eaſily pulled up again, when the platform was to be removed to a new ſituation.

Boning Teleſcope and Rods. Tab. XVIII.

In order to trace the line of 200 yards or 600 feet through the air, from one fixed ſtand to the other, it was uſual, in the firſt place, to ſtretch a cord extremely tight along the ground, and to divide the ſpace into rod lengths, by ſmall wooden pins placed cloſe by the cord, which remained there, and accordingly marked, very nearly, the points over which the centers of the intermediate ſtands were to come. A piece of wood, about fourteen inches in length, and one and a half in breadth, painted white, with a narrow black line along the middle of it, being prepared for the purpoſe, was laid on the ſurface of the farther ſtand. The boning teleſcope, fourteen inches long and one and a half in diameter, with a ſmall magnifying power, and moveable object-glaſs, ſo as to fit it for very ſhort diſtances, was then laid on the ſurface of the nearer ſtand; which, by means of wedges placed under the legs, had that ſide towards the farther ſtand ſo elevated or depreſſed, as to bring the croſs wires to coincide with the black line on the painted board. Twenty-four boning rods had been originally provided; but it rarely happened, that more than eight or ten of

of that number were uſed in any one ſtation. They are of clean deal, upwards of five feet in length, one inch ſquare, and pointed with plate iron at the bottom, ſo as to be eaſily fixed into the ground. Each rod carries a croſs vane, ſix or ſeven inches in length, and three-quarters of an inch in breadth. This croſs vane, being moved upwards or downwards along the rod, till its upper ſurface coincided with the croſs wires of the teleſcope and black line on the painted board, its under ſurface then marked the height to which the ſurface of the ſtand was to be brought at that particular place. In this manner, a certain number of points, in the line paſſing through the air from one fixed ſtand to the other, being accurately obtained, it was very eaſy, at all the intermediate places, by the application of the eye alone to the ſurface of any one ſtand or rod, to bring the ſurfaces of the other ſtands near it into the ſame plane.

Cup and Tripod for preſerving the point upon the ground, where the meaſurement was diſcontinued at night, and reſumed next morning. Tab. XVIII.

It has been already mentioned, and, in giving the account of the rough meaſurement with the chain, there will be farther occaſion to remark, that the baſe was divided into hypothenuſes of 200 yards or 600 feet each, where ſquare pickets were driven into the ground, and regularly numbered, ſo as to be eaſily referred to on any occaſion. In the meaſurement with the rods, it was cuſtomary to finiſh the day's work at or near one of theſe ſtations. When the rods of twenty feet were uſed, the termination of a rod was, of courſe, always found

found to be within a few inches of the picket correſponding with the hypothenuſe, as determined by the chain. But with the rods of twenty feet three inches, the day's work was always ended with a fractional rod, by ſuſpending a plummet from ſome convenient part of the ſtem, marked for the purpoſe, and which conſequently became the point of commencement next morning.

The braſs cup, made uſe of on theſe occaſions, is of the figure of an inverted truncated cone, whoſe mean diameter is four inches, and its depth about five, with a very ſmall inclination in the ſides. It was placed in a hole dug for it in the earth, immediately under the point of ſuſpenſion of the plummet, ſerving only to hold the water in which it vibrated.

The nature of the tripod will be beſt conceived from the plan and elevation of it in tab. XVIII. It conſiſts of two ſtrong pieces of beech wood, mortoiſed into each other, ſo as to reſemble a half croſs, or the letter T inverted, having three ſtrong iron prongs, about twelve inches in length, which paſs through the ends of the wood, and are faſtened to it by ſquare nuts at top. On the ſurface of the tripod lies a ſimilar half croſs of mahogany, moveable by means of grooves in the direction of the longeſt ſide, and fixable by its proper ſcrews, when brought to the deſired poſition. This mahogany half-croſs carries on its ſurface a braſs ruler, moveable at right angles to the former direction, fixable alſo by means of its own ſcrews, and on whoſe end is cut a very fine interſection. Thus any day's operation having been finiſhed, the tripod was placed near the cup, with it longeſt ſide parallel to the line of meaſurement, and its prongs driven into the ground, ſo as to be rendered perfectly immoveable without great violence. The plummet being then ſuſpended by a fine gilt wire, at any part

of

of the ſtem of the deal rods indifferently, but always at the fixed * or hindermoſt end of the glaſs rods, the braſs ruler was advanced ſo near as almoſt to touch the wire, and there made faſt. This being done, the mahogany half-croſs was laſtly moved backwards or forwards, in the direction of the line of meaſurement, until the interſection, as ſeen by a perſon lying down on the ground for the purpoſe, accurately coincided with the gilt wire, where it was likewiſe faſtened by its proper ſcrews. A tent was then pitched very near the apparatus, for the ſoldiers who furniſhed the centinel for its ſecurity, till the meaſurement was reſumed; and particularly to guard it from being diſturbed by cattle during the night.

Wheels for terminating, in a permanent manner, the extremities of the Baſe. Tab. XVIII.

Before any accurate meaſurement could ultimately be made of the baſe by means of rods, in order that we might with certainty refer to the ſame point, on any occaſion that might ariſe of correction or repetition of the work, it had all along been foreſeen, that it would be abſolutely neceſſary to ſink deep into the ground wooden pipes, or ſuch like things, at the extremities of the baſe, which could not be removed, or even diſturbed, by idle or ignorant people, without very conſiderable labour. Mr. Mylne, F.R.S. was accordingly requeſted to

* That this might be conveniently done, a moveable ſtand was placed, under the glaſs rod, about four feet from the fixed end, and its table elevated till, by bearing againſt the lower part of the caſe, it received its weight. This permitted the ſtand under the fixed end to be lowered and removed, to make room for the apparatus.

order

order two ſuch pipes to be provided, about ſix feet in length each, and one foot in diameter, with a bore of four inches in the uppermoſt end, for the depth of two feet, and croſs-arms near the lowermoſt end, in the ſtile of the common warping poſts. As an improvement on this idea, Mr. MYLNE very judiciouſly propoſed that, inſtead of the croſs-arms, the lower ends of the pipes ſhould paſs through the nave of an old coach-wheel, and then be ſecured by a bolt underneath. This alteration was approved of; and the machines, thus executed, were ſent ſoon after by water to Hampton.

The plan and ſection of one of theſe wheels, with the diſhed ſide downwards, are repreſented towards the left-hand in tab. XVIII. where it will be perceived, that by means of four knee-pieces, made of crooked oak, the pipe is firmly bolted to the wheel, and thereby kept at right-angles to its plane. The top of the pipe is alſo ſecured exteriorly by an iron hoop, and has a caſt-iron box driven into it, whoſe inner diameter is four inches, anſwering to that of the bore. Four oak piles for each wheel were prepared to be driven into the bottoms of the pits dug for their reception, which were ſix feet in diameter, and the ſame in depth. The ſoil near Hampton Poor-houſe being of a looſe ſandy nature, there the piles were eaſily driven into the bottom, until their tops were on the ſame level. The flat of the fellies of the wheel being then laid on the piles, the earth was filled in and well rammed around the pipe, quite up to the ſurface, with which its mouth is even. But the ſoil at King's Arbour, being a hard-bound gravel, the piles could not be driven into the bottom of that pit; wherefore, the flat of the wheel reſts there on the gravel only.

The braſs cup, formerly deſcribed, was from the firſt intended to be placed in the pipes, for which purpoſe it has two

lids; one a ſemi-circle, with the central point marked by a line cut on its diameter, brought into the direction of the baſe; with which line the gilt wire, ſuſpended at the extremity of the firſt rod, was made to coincide on the commencement of the meaſurement. The other lid has a very ſmall hole made in its center, through which the plummet wire is to paſs, when ſuſpended from the center of the inſtrument, hereafter to be made uſe of for the determination of the angles at the baſe, or in any other ſtation whatever, where it may be neceſſary to bring it very accurately over a point on the ſurface of the ground underneath.

Rough meaſurement of the Baſe with the Chain, and determination of the relative heights of the Stations by means of the Teleſcopic Spirit Level. Tab. XVI. and XVII.

Having in the preceding deſcription of the various inſtruments, originally provided for the meaſurement of the baſe, fully explained their conſtructions, uſes, and modes of application; and having thereby anticipated, in a great degree, what muſt otherwiſe have been ſaid to make them underſtood in any account, blended with that of the execution; little more now remains to be given than the journal of our proceedings from day to day, and the ultimate reſult of the operation.

After a very tedious delay, Mr. Ramsden having at laſt produced his hundred-feet chain, with the portable tranſit inſtrument; and having lent us an excellent teleſcopic ſpirit level, for determining the relative heights; two ſections of the baſe being likewiſe cleared by the ſoldiers, and ſome progreſs made in

the third, we found ourselves, on the 16th of June, in readiness to begin the rough measurement.

Lieut. Colonel CALDERWOOD, of his Majesty's Horse-Guards, F.R.S. had, from the beginning, been so good as to promise his assistance in the operation. Lieut. Colonel PRINGLE too, of the Corps of Engineers, obligingly became a volunteer on the occasion; as did also Mr. LLOYD, F.R.S. a few days afterwards; while Ensign REYNOLDS, of the 34th regiment, who had for some time past been employed in surveying the environs of the Heath, continued that work with such spare hands as could be afforded him for that purpose; and it is to the plan (tab. XVI.) done by that officer, that it will be necessary to refer in any thing regarding locality, in what has hitherto been said, as well as in the subsequent relation.

The lower end of the base had for some time past been distinguished by a St. George's flag fixed to the top of a fir spar, thirty-five feet in height; and one of the signal bell-tents still remained at the station near the summer-house. A rope of 200 yards being made very fast by a strong iron picket, driven into the ground at the bottom of the flag-staff, the other end was carried on along the base, and placed at the bottom of a camp-colour, in a line with the bell-tent. The rope being wound around a strong iron reel, prepared for the purpose, was thereby stretched extremely tight, a person occasionally lifting it up in the middle, or at other places, and letting it drop again, so as to bring the whole into the same straight line. Five persons were necessary for the proper management of the chain; two at each end for its adjustment there, and one towards the middle, to lay it close to the rope, or to bear it up in any particular place, where the circumstances of the ground rendered such precautions useful. The zero or rear end of the chain

being ſtrained back ſo as to coincide with the point of commencement, a ſteel arrow was placed as erect as poſſible in the ſemi-circular cavity of the braſs handle at the other end. The chain being then drawn on, till the cavity in the rear handle could be applied to the firſt arrow, a ſecond was then placed in that of the front handle, and ſo on until ſix chain lengths were thus meaſured off; which terminating the firſt hypothenuſe, a beech picket, ſomething more than an inch ſquare, and about ſeven in length, with N° 1. cut upon it, was driven into the ground, till its head was nearly level with the ſurface. It is however to be remarked, that the ſixth arrow of each hypothenuſe was conſtantly left in the ground till the firſt of the ſucceeding one was placed, to avoid the error that would have otherwiſe ariſen in applying the rear end of the chain to the picket inſtead of the arrow.

In this manner we proceeded on the 16th of June, and in the ſpace of about three hours and a half, completed the firſt meaſurement of the ſouth-eaſt ſection of the baſe, comprehending the thirteen hypothenuſes between the flag-ſtaff and ſtation near Hanworth Summer-houſe, the diſtance being 78 chains or 7800 feet, making 2600 yards; and the mean temperature of the air being 63°.

On the ſubſequent day this ſection was re-meaſured with equal care, when the total extent fell ſhort of the thirteenth picket only five inches. And here it is to be obſerved, that a conſiderable part of this difference probably aroſe from the ſtretching of the chain acroſs Wolſey River, at the ſame time that the irregularities of the ground are greater in this than in either of the other two ſections. The mean heat of this day was 65°.

The

The operation with the chain was ſuſpended during the 18th and 19th of June, thoſe days having been employed in ſettling certain matters with Mr. RAMSDEN relative to the deal rods, as well as to give time for the making of a holdfaſt for the rear end of the chain, invented by Lieut. Colonel PRINGLE. This machine, whereof the plan at large is repreſented by dotted lines at the handle of the chain, as it is in ſmall by the two elevations adjoining in tab. XVII. conſiſts of a ſemi-circular iron plate, from the bottom of which projects two double and one ſingle prong. In the middle, between the two double prongs, a ſemi-circular cavity is formed, fitted to receive the ſteel arrow on one ſide, while that in the braſs receives it on the other. In a ſocket in the middle, a ſtrong wooden handle, reſembling that of a ſpade, is placed. Thus the rear handle of the chain being applied to the arrow, the holdfaſt embraces with its double prongs the ſtraight part of the braſs, and in that poſition, being forced into the ground by the action of a man at the handle, the rear end of the chain is thereby kept ſo firm as to be immoveable by the efforts of the two men at the other end, in ſtretching it to its true poſition, for the front arrow.

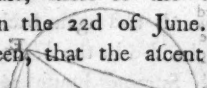
On Monday, the 21ſt of June, the operations were reſumed, by meaſuring twice with the chain (forwards and again backwards) the thirteen hypothenuſes comprehended in the ſecond ſection of the baſe, between Hanworth Summer-houſe and the north-weſt bank of the great road (an old Roman way) leading from Staines to London. This being the ſmootheſt part of the Heath, and the holdfaſt being now applied, the two meaſurements differed only one inch and a half in the diſtance of 7800 feet. This inſtance of accuracy is alone ſuf-

ficient

ficient to prove the great excellence of the chain, although another will be given hereafter ſtill more ſurpriſing.

On the ſame day that the ſecond ſection of the baſe was meaſured, the levels of that and the firſt were taken. The operation of levelling is ſo univerſally known, as to render any detail of it unneceſſary. It will be ſufficient to ſay, that the ſpirit level made uſe of on this occaſion was a very good one, about eighteen inches in length, and could at all times be very readily and accurately adjuſted by inverſion in its Y's. The tops of the pickets, marking the hypothenuſal diſtances, were the points on which the levelling rods were placed on each ſide of the level; which being inverted at the intermediate picket, points equi-diſtant from the center of the earth were thereby obtained, at the croſs vanes of the levelling rods, and no correction for curvature or refraction neceſſary. It will be readily underſtood, that the relative heights of the pickets were found by meaſuring their diſtances from the centers of the croſs vanes and axis of the teleſcope reſpectively.

The ſix firſt columns towards the left-hand of the firſt or general table ſubjoined to this Paper, ſhew diſtinctly every thing relating to the levels of the whole baſe, thoſe of the third ſection having been determined on the 22d of June. By examining the table it will be ſeen, that the aſcent on the firſt ſection is 10.555 feet,



on the ſecond . . . 8.580
and on the third . . . 12.130

Total . . . 31.265 feet, between the lower extremity at Hampton Poor-houſe, and the higher near King's-Arbour.

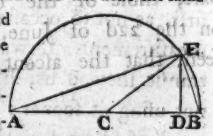
The

The computed numbers in the ſeventh column are the reductions * depending on the aforeſaid heights, or the differences between the hypothenuſal diſtances of 600 feet each and the reduced baſe diſtances. With regard to the remaining columns of the table, or thoſe towards the right-hand, they will be ſeverally ſpoken to hereafter, in taking into conſideration the expanſion of metals, as determined with great accuracy by the experiments with the pyrometer.

Hitherto no uſe had been made of the tranſit inſtrument: for, in order that it might be applied to advantage, there was a neceſſity for laying the wheel into the ground at the lower end of the baſe, and ſo to modify the St. George's flag-ſtaff that, being placed in the pipe, it might be ſteadily ſupported by braces in a true vertical poſition; which we found, from experience, could not be effected by ropes only.

The wheel being accordingly laid in its place, and the other precautions taken for ſecuring the flag-ſtaff, which was likewiſe painted white, that it might be more diſtinctly ſeen from

* The reduction in the ſeventh column, I have computed by the difference between the ſquare of the hypothenuſe, actually meaſured, and the ſquare of the height found by the level; and Lieut. Colonel CALDERWOOD has done the ſame thing by a much ſhorter method. Thus, in the annexed figure, CE being the hypothenuſe of 600 feet, DE the perpendicular height obtained by levelling, DB the reduction required, or the difference between the hypothenuſe and true baſe; then, ſubſtituting the chord BE inſtead of DE, the following analogy is obtained, AB : BE :: BE : DB; conſequently, $\frac{BE^2}{AB} = DB$: that is, the ſquare of the perpendicular height being divided by double the diſtance, or 1200 feet, the quotient is equal to DB the reduction, without ſenſible error. For if DE were four feet, the greateſt perpendicular height in the baſe, BE the chord would only exceed it $\frac{1}{200000}$, which would not be more than $\frac{1}{1000}$ part of an inch. The difference between the reſults, by the two modes of computation, is ſo trifling as not to deſerve notice.



the

the farther extremity; on the 22d of June, the tranſit inſtrument was adjuſted over the thirteenth picket at Hanworth Summer-houſe, while directed upon the flag-ſtaff. But it being now found, that the vertical plane paſſing through the flag-ſtaff fell to the eaſtward of the center of Banſted Spire, therefore the tranſit was gradually moved to the eaſtward, until by repeated trials the three points were perceived to be in the ſame vertical plane, when the picket was moved, and re-placed exactly under the axis of the teleſcope, a few inches from its firſt poſition. The ſame operation was repeated at the twenty-ſixth ſtation, on the farther bank of the Staines Road; and, laſtly, at the forty-ſixth, forming the north-weſt extremity of the baſe; where a pit was immediately dug for the wheel, which was placed therein, without however filling in the earth for the preſent, that being deferred till near the completion of the meaſurement with the deal rods. Thus the two extremities, and two intermediate points of the baſe, being accurately placed, by the help of the tranſit inſtrument, in the ſame vertical plane with Banſted Spire, it was eaſily ſeen, that by arranging camp colours in the intervals at any time, all the other points might be brought ſo nearly to coincide with theſe firſt, as not to occaſion, by deviation, any ſenſible error in the meaſurement afterwards to be made. This application of the tranſit ſhewed us, however, that ſome labour had been loſt by not uſing it ſooner: for at the Staines Road, the tract cleared by the ſoldiers deviated about two feet and a half too much to the weſtward for the true line; and at King's Arbour it was twice as much; ſo that we were now obliged to widen the cleared tract, by adding to the eaſtern ſide of it.

On the ſame day that the chief points in the baſe were fixed by means of the tranſit, and the levels of the third ſection

taken

taken as before-mentioned, the rough measurement of that section with the chain was completed, and found to contain nineteen hypothenusal distances of 600 feet each, and one of 404.55, making in the whole 11804.55 feet, between the twenty-sixth station at the Staines Road and the center of the pipe near King's Arbour, the mean temperature being 62°½. Here it is to be observed, that this last section was only measured once with the chain, the tract not being yet sufficiently cleared to admit of its being done to the best advantage; and, when completed, it was judged to be better to proceed directly in the operation with the rods, than to lose time in the usual repetition, since the merits of the chain, in this way of applying it, were already sufficiently well established; and any future tests to which it was to be put were proposed to be of a more rigid nature.

When the length of the chain, in its original state, was ascertained by the dots on the brass pins in the New-England plank, it was found, in the then temperature of 74°, to exceed the 100 feet by near one quarter of an inch, or 0.245 inch. Therefore, in the temperature of 63°, being that in which the lengths of the deal rods were laid off, and differing very little from what was likewise the mean heat of the air, when applied upon the Heath, the chain, according to the experiments on the expansion of the very same steel, would exceed the 100 feet by 0.161 inch, or 0.0134 foot. Hence the sum of the three sections of the base, 274 chains, being multiplied by 0.0134 foot, we shall have 3.67 feet for the equation of the chain + 4.55 feet, to be added to its length, which will then become 27408.22 feet from the center of one pipe to the center of the other: and this would have been the true length of the base, as given by the rough measurement with the

chain, if the ſurface had been one uniform inclined plane throughout its whole extent. But, although the aſcent of Hounſlow-Heath is ſo ſmall, and ſo gradual, as to occaſion little more than half an inch of reduction, from the 46 hypothenuſal to the 46 baſe diſtances, into which it is divided, as may be ſeen by referring to the table; yet each of theſe hypothenuſes containing again many other ſmall irregularities, all of which affect the meaſurement by the chain, in proportion to their number and height, in every ſpace of 600 feet, their united effects, including the lateral deviations from the true line in meaſuring, do ſomewhat more than compenſate for the extra-length of the chain, as will be ſeen hereafter in comparing the length of the baſe juſt now obtained with that given by the rods.

The weather, which during the greater part of June had been wet, became ſtill worſe towards the end of the month and firſt week of July; ſo much ſo, that even if the deal rods had been ready they could not have been uſed with advantage. The ſoldiers, nevertheleſs, were not idle, being, when the weather would permit, partly employed in clearing the Heath, and partly in aſſiſting Mr. REYNOLDS in the ſurvey, towards the perfecting of which many chief points were fixed by means of my aſtronomical quadrant, placed for that purpoſe at ſeveral different ſtations of the baſe. At this time too (July 8th) I levelled from the lower end of the baſe to the ſurface of the Thames at Hampton, and found the deſcent to be 36.1 feet.

Meaſurement

Meaſurement of the Baſe with the Deal Rods. Tab. XVI. and XVIII.

Such extraordinary care and pains had been beſtowed in the conſtruction of the deal rods, in order to render them the beſt which had ever been made, that, although begun early in June, they were not completely finiſhed before the 15th of July. They were brought that afternoon by Mr. RAMSDEN, together with the various parts of the apparatus neceſſary for their application in the field, to the camp now moved from Hanworth Summer-houſe to the interſection of the baſe with Wolſey River; whence they were tranſported, early next morning, to the pipe near Hampton Poor-houſe, where we were met by Sir JOSEPH BANKS, accompanied by Meſſ. BLAGDEN, CAVENDISH, LLOYD, and SMEATON, all ready to lend their aſſiſtance in the ſubſequent menſuration.

Before I proceed farther, I think it here incumbent upon me very gratefully to remark, that the reſpectable and very worthy Preſident of the Royal Society, ever zealous in the cauſe of ſcience, and who had repeatedly viſited the heath, to offer aid, if ſuch had been neceſſary, while the firſt and rougher part of the operations were going on; now, that others of a more delicate nature were to commence, and where it was of importance, that thoſe entruſted with the execution ſhould meet with as few, and as ſhort, interruptions as poſſible, not only gave his attendance from morning to night in the field, during the whole progreſs of the work; but alſo, with that liberality of mind which diſtinguiſhes all his actions, ordered his tents to be continually pitched near at hand, where his immediate

gueſts, and the numerous viſitors whom curioſity drew to the ſpot, met with the moſt hoſpitable ſupply of every neceſſary and even elegant refreſhment. It will eaſily be imagined, how greatly this tended to expedite the work, and how much more comfortable and pleaſant it rendered the labour to all who obligingly took part in it; but more eſpecially to him, who, being a volunteer in it at firſt, conſidered himſelf as bound to perſevere in his beſt endeavours to bring it to a ſucceſsful concluſion.

From the deſcription that has been given of the deal rods, it will be remembered, that they are fitted to be applied in meaſuring, either by the coincidences of lines, inlaid one inch and a half from each extremity, or by the contacts of the ſpherical lips of the bell-metal with which they are tipped. The firſt, ſeeming to be the moſt accurate, although the moſt tedious method, was that by which we propoſed to ſet out.

The flag-ſtaff having been previouſly removed from the pipe, and the braſs cup filled with water put in its ſtead, all the neceſſary precautions being likewiſe taken for preſerving the line of direction, horizontally, by the rope ſtretched along the firſt hypothenuſe, and vertically, by means of the boning rods; the firſt ivory line on the firſt rod was brought by the plummet to coincide with the center of the cup, in which poſition, being clamped, it accurately marked the commencement of the baſe. The ſecond rod being now applied to the firſt, and moved up by the apparatus formerly deſcribed (tab. XVIII.) till its line coincided with that on the firſt; and, in like manner, the third rod being applied on the alternate ſide of the ſecond, moved up and clamped as the reſt; thus the exact diſtance of ſixty feet was aſcertained, care being always taken, that the firſt adjuſtments were not diſturbed, while the

ſubſequent

ſubſequent ones were forming. The clamps faſtening the firſt rod to its ſtands being then detached, it was carried by two men and laid on the alternate ſide of the third; and ſo on in ſucceſſion, until fifteen rod lengths were meaſured off, being the half of the firſt hypothenuſe.

The time conſumed in meaſuring this ſhort diſtance of 300 feet was not leſs than five hours; owing, as has been formerly mentioned, to the confined nature of the apparatus for moving the rods on into coincidence, which required ſuch nicety in placing the ſtands, as could not be effected until after ſeveral repeated unſucceſsful trials. All the executive people were therefore of opinion, that it would be proper to diſcontinue this mode of meaſurement, at leaſt until a more convenient apparatus could be thought of for the purpoſe; and that, in the mean time, we ſhould proceed by the method of contacts, as the only alternative we could for the preſent adopt *.

The rods being accordingly placed in contact with each other, we ſoon made greater progreſs, finiſhing the operations of the day at the middle of the fourth hypothenuſe, where the tripod, with its guard, was placed, to preſerve the point of commencement for the enſuing morning.

* Although I acquieſced in the change thus become neceſſary, yet it was with much reluctance, becauſe it left undecided the conteſted point, with regard to coincidences and contacts. If we could have proceeded with the coincident rods till eighty-one lengths were meaſured off, and then meaſured back the ſame ſpace by placing eighty rods in contact, the point would have been clearly ſettled. For if the termination of the eightieth rod agreed exactly with the point of departure, contacts being the moſt expeditious would have been judged the beſt method. On the contrary, if the eightieth rod fell ſhort of reaching the point of departure, there could have been no doubt, that the difference muſt have ariſen from butting one rod againſt the other, whereby a certain ſmall proportion of each rod came to be loſt in the account, by being meaſured twice over.

The

The measuring rods, when put into the chest in London, had been compared and found to agree with the standard. The comparison was not repeated on the 16th; but this being done on the 17th, at 7 h. A.M. under the oil-cloth canopy at the camp, they were found at a medium to exceed the standard by one-fiftieth of an inch, the temperature then being 62°. After the comparison they were carried to the place of the tripod, when the operation was resumed by bringing, with the help of the plummet, the same point of the rod with which we had left off work, to coincide with the intersection on the brass ruler. The measurement of this day was closed at the end of the tenth hypothenuse, when the rods being carried back to camp, were compared, and found accurately to agree with the standard.

A considerable fall in the barometer, between the evening of the 17th and the morning of the 19th, portended rain. Nevertheless, all parties repaired to the place of rendezvous, which was appointed at the lower end of the base, in order to re-measure the two first hypothenuses, by placing all the rods in contact, which on the 16th had been done partly one way and partly the other. The operation being according repeated with great care, the point of the sixtieth rod, which formerly corresponded to the center of the second picket, was now found to be pushed forward exactly forty-five inches, answerable to the deficiency on the fifteen coincident rods, with which the mensuration was begun. It now began to rain, therefore the rods were carried back to camp, and being severally compared, they were found to exceed the standard each by one-thirtieth of an inch, occasioned by the extraordinary humidity of the air. A heavy rain ensued; and what made this much more regretted by all was, that in the forenoon their Majesties graciously condescended to honour the camp with their presence, and continued

tinued there some time; but the weather becoming rather worse, it was utterly impossible to shew their Majesties the nature of the operation, by any progress that could at that time be made in the work.

After a continuance of unfavourable weather for several days, the operations were resumed at 9 h. A.M. of the 23d, when the rods being compared were found still to exceed the standard by one-thirtieth of an inch, and the temperature now was 61°. Here it is to be observed, that in our progress forward, an accurate register had been all along kept of that point of each rod corresponding to the center of the hypothenusal pickets, by noting its distance from either end, whereby the error of the chain at each station was readily discovered, at the same time that the revolutions of the three rods served to keep the account of the total measurement. In order, therefore, that this method might be distinctly adhered to, it was judged proper to push on the rod that lay over the tripod at N° 10. exactly forty-five inches, to make good the deficiency of the first fifteen coincident rods, and that the account might be kept from the lower end of the base in entire rods of 243, and complete revolutions of 729 inches each. This being done, the rest were placed in the ordinary succession; and we finished the business of the day at the eighteenth station, where the rods being compared at 6 h. P.M. their mean length was found to exceed that of the standard $\frac{1}{67}$th part of an inch, the temperature then being 54°.

On Saturday the 24th of July, the rods were three times compared; at 7 h. 30′ A.M., 11 h. 15′ A.M., and 5 h. 45′ P.M. Their mean excess above the standard was found to be one-thirtieth of an inch, and the mean heat 64°. In the course of the day, the measurement was continued from the eighteenth to the

the twenty-ſeventh ſtation, or firſt of the third ſection of the baſe, where the tripod was placed as uſual; and there it remained untouched, on account of bad weather, till Monday the 2d of Auguſt.

Conſidering how much time and labour had been beſtowed in obtaining what we certainly had every reaſon to conclude were the beſt deal rods that ever were made, it was no ſmall diſappointment now to find, that they were ſo liable to lengthen and ſhorten by the humid and dry ſtates of the atmoſphere, as to leave us no hopes of being able, by their means, to determine the length of the baſe to that degree of preciſion we had all along aimed at. But ſince more than one-half of it was already meaſured, it was judged proper to proceed with them in their preſent ſtate, and then to have them carefully painted or varniſhed, before they ſhould be farther uſed.

The unfavourableneſs of the ſeaſon, and delays in obtaining the inſtruments, had already been the cauſes of protracting the operations on Hounſlow-Heath greatly beyond what was at firſt expected; and the failure of the deal rods gave no immediate proſpect of their being ſpeedily brought to a concluſion. On revolving in my own mind the different alternatives we might ultimately be obliged to have recourſe to, metal rods of ſome kind or other, whoſe expanſion could always be determined by experiment, ſeemed to promiſe a reſult that might be ſafely relied on. Caſt iron was what I had thoughts of propoſing, knowing from an experiment which I had made myſelf, that it expanded leſs than ſteel. The cumberſomeneſs of its weight appeared indeed objectionable; but that inconvenience was either to be ſubmitted to, or one of another kind, namely, the reduction of the length, which was always, if poſſible, to be avoided.

At this time Lieut. Colonel CALDERWOOD could not conveniently lend us his assistance in the field; but he visited us occasionally, and on one of these visits proposed to me, that glass rods should be made use of instead of deal; putting me in mind of another experiment * that I had made, which seemed to shew that solid glass rods expanded less than tubes. This proposition the Lieutenant-Colonel, before he came to the heath, had made to Mr. RAMSDEN, who appeared averse from making the trial, because of the great length of the rods, and the brittleness of the material. Nevertheless, it being sufficiently obvious, that glass rods or tubes of the full length, or something approaching towards it, would be much sooner provided than any metal rods whatever, and the saving of time being a point of consequence; Lieut. Colonel CALDERWOOD was accordingly requested to make the trial at the glass-house, as soon as possible after his return to town. Next day he succeeded in getting a fine tube drawn, eighteen feet long, and about one inch in diameter; and there seemed to be no longer any doubt, that those of the proper length might be obtained. It was found, that solid glass rods of such extraordinary dimensions could not be had, it being impossible to take at once a sufficient

* The experiment here alluded to was made with Mr. CUMMING's pyrometer, which from its construction did not admit of a very accurate estimation of the heat communicated to the standard bar, the rod, and tube respectively. Either, therefore, the natures of the glass rod and tube, made use of at that time, must have been very different, to cause the difference of expansion; or some circumstance in the instrument unattended to had occasioned the fallacious appearance: for it will be found, from the experiments hereafter to be given in detail, that a solid glass pendulum rod expands fully as much as, nay in this particular instance even more than a tube; but different glass, having different specific gravities, will no doubt be susceptible of different degrees of expansibility.

quantity of the melted metal on the irons, made uſe of for drawing them at the glaſs-houſe.

The week of rainy weather, which ended the month of July, occaſioning, as has been ſaid, a total ſuſpenſion of the operations on the heath, was employed in procuring a ſufficient number of glaſs tubes (one whereof was not leſs than twenty-ſix feet long) and regulating with Mr. RAMSDEN every thing concerning their conſtruction into meaſuring rods. The deſcription of them we ſhall however defer until the time of their application in the field, after having finiſhed the operation with thoſe of deal.

On Monday the 2d of Auguſt, the operations on the heath were reſumed at 8 h. 30′ A.M. by comparing the rods with the ſtandard, which they were found to exceed by one-fortieth of an inch, the temperature then being 66°. The forward end of the rod now placed over the tripod at N° 27, completing the 800th length, reckoned from the lower end of the baſe by rods of 243 inches each; and theſe being equal to 810 rods of 240 inches; it was judged proper to mark a point upon the ground correſponding to this forward end, that it might be referred to in returning back with the meaſurement by the glaſs rods. This was done by ſinking two ſmall pickets into the ground, about a foot aſunder, one on each ſide of the baſe, and at right angles to it. A ſilk thread being then ſtretched over the tops of the pickets, and gently moved on till it touched the ſilver wire ſuſpended from the end of the rod, fine notches were then made with a pen-knife in the tops of the pickets, whereby the thread could be replaced in the ſame ſituation; which being done, the pickets were covered over with earth. In the courſe of this day nine hypothenuſes were meaſured; and at 7 h. P.M. the tripod was placed at the thirty-

ſixth

sixth station. The rods, being now compared, were found to agree with the standard; and the temperature was 67°¼.

On Tuesday the 3d of August, the rods were compared at 7 h. A.M. and found only to exceed the standard by one-sixtieth of an inch. Being arrived at the middle of the forty-first hypothenuse, a point corresponding to the forward end of the 1215th rod was transferred to the ground by the double pickets and silk-thread, as had been done at the twenty-seventh station. The measurement was then continued to the north-west extremity of the base, which was found in the whole to contain 1353 complete long rods of 243 inches each +21 inches, where the tripod was placed, in the point which of course corresponded to the 1370th short rod of 240 inches each, equal to 328800 inches, or 27400 feet. To which distance we have yet to add 4.31 feet, being the space intercepted between the intersection on the tripod and the center of the pipe marking the north-west extremity of the base; whose total length, as given by the deal rods, without regard to expansion, or reduction of the hypothenusal line, becomes 27404.31 feet. And here it is to be observed, that the intersection on the tripod terminating the 27400 feet only over-shot the picket answering to the 274th chain by two inches and nine-tenths. But this nice agreement between the result by the deal rods, and that furnished by the rough measurement with the chain, arises from the extra-length of this last, which so nearly compensated for all the irregularities of the surface.

The measurement with the deal rods being finished, they were compared at 5 h. PM. and found to agree with the standard, the temperature then being 75°.

Expanſion of the Deal Rods.

It has been an opinion generally enough, although, as we have ſeen, erroneouſly received, that very ſtraight-fibred deal was not at all, or but little, affected longitudinally by the humidity of the air. That we might not be led aſtray by truſting to fallacies of this ſort, the ſtandard rod had been provided; which being always cloſely ſhut up in its cheſt, except during the ſhort interim of compariſon, could feel but a ſmall proportion of the effects which the meaſuring rods ſuffered, theſe being conſtantly expoſed to the open air throughout the day, as well as to the moiſture of the night, when lying under the oil-cloth canopy. The ſtandard rod, it is true, could not be accurately compared with the braſs ſcale: for although when conſtructed, braſs pins, forty inches aſunder, had been driven into its ſtem, for the purpoſe of ſuch compariſon, yet theſe had afterwards been diſplaced, or at leaſt the points upon them defaced, by the planing over of the upper ſurface. This circumſtance, which was unattended to when the operations commenced, is now of no conſequence; becauſe, from an experiment hereafter to be mentioned, the lengthening of the ſtandard may be pretty nearly aſcertained. But ſince there are ſome contradictory circumſtances, ſoon to be mentioned, in the operation with the deal rods, which would have made a repetition of it abſolutely neceſſary, if we had not now obtained thoſe of a different kind, ſo very unexceptionable in their nature and mode of application, as, in the preſent caſe, to admit of no competition between the two reſults, and to render it improper on our part ever to have farther recourſe to the firſt; ſo there

can

can be little doubt, that deal rods will be universally rejected by other countries, in any measurements they may have occasion to make in future.

About the 10th of July, two rods, one of New-England and the other of Riga deal, being measured by the fixed points in the great plank in Mr. RAMSDEN's shop, and having each two brass pins driven into them at the distance of twenty feet, were laid on the top of the house, where they remained until the 26th, the weather, for the greater part of the time, having been very wet. They were then taken down, and being, by means of the long beam compasses, compared with the measures on the plank, the New-England rod was found to have lengthened 0.031 inch, and the Riga rod 0.041 inch. By which experiment the fact seems to be established, that Riga red wood, notwithstanding the quantity of turpentine which it contains, is more susceptible of the effects of moisture than New-England white wood. Mr. RAMSDEN likewise finds, that the great plank so often mentioned, suffers, in ordinary summer weather, an alternate expansion and contraction, amounting at a medium to 0.0041 of an inch every day: that is to say, if the distance between the twenty-feet brass points be measured from the scale, by means of the beam compasses, in the evening, it is found to have lengthened next morning 0.0041 of an inch, by the humidity of the intervening night. In the course of the following day it contracts again to its former length, and so on. Mr. RAMSDEN has often observed this alternate change in the deal planks; but it was particularly on the 11th and 12th of August, that the quantity was actually measured. It will readily be understood, that any difference of temperature which might have happened in the brass scale, at

the times of compariſon, was always carefully taken into the account.

Now, from this laſt experiment, it ſeems probable, that we ſhall not be very wide of the truth in ſuppoſing, that the ſtandard deal rod, which lay cloſed up in its cheſt, under the canopy on Hounſlow-Heath, would ſuffer the ſame ſort of alternate expanſion and contraction with the above-mentioned plank; that is to ſay, being of Riga wood, its mean expanſion about the middle of the day would be $\frac{25}{10000}$ of an inch. By this quantity then we muſt augment the actual obſerved expanſion of the meaſuring rods, in order to obtain within certain probable limits (ſince we cannot determine it accurately) the equation for the expanſion; or that ſpace by which the apparent meaſurement, given by the 1370 deal rods, ſhould be augmented in order to obtain the true length of the baſe; or that which would have been given by unalterable rods, of the ſame original length with thoſe of deal, as expreſſed in the following table.

Table

Table of the Expanſion of the Deal Rods.

Days.	N° of rods meaſ.	Hour of compariſon.	Temp. of the air.	Obſerved expanſion.	Decimal mean.	Equation for the meaſ. rods.	Equation for the ſtandard.	Total expanſion.
		h. ′	°	In.		In.	In.	In.
July 16	105	4 0A.M.	48	$\frac{1}{100}$th	0.010	1.050	0.2625	1.3125
		6 0P.M.	62	0				
17	195	7 0A.M.	62	$\frac{1}{100}$th	0.010	1.950	0.4875	2.4375
		6 0P.M.	—	0				
23	240	9 0A.M.	61	$\frac{1}{70}$	0.021	5.040	0.6000	5.6400
		6 0P.M.	54	$\frac{1}{67}$				
24	270	7 30A.M.	61	$\frac{1}{56}$	0.033	8.910	0.6650	9.5750
		11 15A.M.	66	$\frac{1}{71}$				
		5 45P.M.	64	$\frac{1}{73}$				
Aug. 2	270	8 30A.M.	66	$\frac{1}{70}$	0.0125	3.375	0.6650	4.0400
		7 0P.M.	67½	0				
3	290	7 0A.M.	56	$\frac{1}{69}$	0.017	0.493	0.7230	1.2180
		5 0P.M.	75	0				
Total	1370	— —	— —	— —	— —	20.818	3.405	24.223

N. B. Although the rods were not compared with the ſtandard on the 16th of July, yet the expanſion probably was, and therefore has been eſtimated, at the ſame rate as it was found on the following day.

By examining the preceding table, it will appear, that the total expanſion on the 1370 deal rods, including the ſmall equation for the lengthening of the ſtandard, amounts to 24.223 inches, or 2.02 feet; which being added to the apparent length of the baſe 27404.31 feet formerly obtained, we ſhall have, for the hypothenuſal length, 27406.33 feet: and from this deducting 0.07 foot, the exceſs of the hypothenuſal above the baſe line, or the reduction contained in the ſeventh column of the general table of the baſe, there will remain 27406.26 for the diſtance given, by the deal rods, between the

centers of the pipes terminating the baſe, reduced to the level of the loweſt, or that at Hampton Poor-houſe, in the temperature of 63°, being that of the braſs ſcale when the lengths of the deal rods were laid off. All this, however, ſuppoſes three things to be abſolutely certain: firſt, that the expanſion of the rods has been accurately eſtimated; ſecondly, that no error has ariſen from the butting of the rods againſt each other, in order to bring them into contact; and, thirdly, that no miſtake of any kind has been committed in the execution. When we come to give the true length of the baſe, as ultimately aſcertained by means of the glaſs rods, it will appear, that one or more of theſe three have actually taken place; although it is moſt probable, that only the two firſt ſources of error have contributed their ſhare of the total difference between the two reſults. But the diſcuſſion of this point muſt be deferred for the preſent; and I ſhall now finiſh the ſubject of the expanſion of the deal rods, by mentioning two other compariſons of them, which ſerve to ſhew ſtill more obviouſly, how improper they are for very accurate meaſurement!

It has already been remarked, that the laſt week of July was ſo wet as to occaſion a total ſuſpenſion of the operations on Hounſlow-Heath. On the 26th of that month, at 8 h. A.M. the temperature being then 62°, the rods were compared with the ſtandard, and found to exceed it, at a medium, one-fifteenth part of an inch. Now, if we ſuppoſe the whole baſe to have been meaſured with the rods in that ſtate, the difference would have amounted to more than 7½ feet, excluſive of what the ſtandard itſelf might have altered from its original length.

The other compariſon was made at Spring-Grove, in the beginning of September, after our operations on the heath had been finiſhed, and the deal rods with their apparatus depoſited

under the roof of Sir JOSEPH BANKS's Barn. The object here in view was the measurement of such a space as the garden would conveniently admit of, when the rods were in their dry or contracted state; and to re-measure the same space next morning, when the rods, being left out for the purpose, had imbibed all the humidity they could from the moisture of the intervening night. Accordingly, the fourth being a fine dry day, the sun shining bright, and the thermometer about 68°, seventeen stands were arranged in the long walk, with so much nicety in the same inclined plane as to appear but like one. The first or lowermost stand had a brass cock screwed to its top. The two uppermost, that is to say, the sixteenth and seventeenth, were of the fixed kind, each with a brass slide, and placed only forty-five inches asunder. The first deal rod was made to butt against the brass cock, and the rest successively against each other, until fifteen rod lengths were measured off, and a fine line drawn on the slide marking the extremity of the fifteenth. That rod being removed, forty-five inches, taken from the brass scale, were then laid off backwards from the line on the slide of the seventeenth to the slide of the sixteenth stand, where another fine line was drawn. Thus the space comprehended between this last line and the cock on the first stand, was just 300 feet, or fifteen coincident rods. During the night of the 4th, which was very fine, the rods lay on the smooth grass. About sun-rising of the 5th there came on a thick fog, which entirely dispelled about 8 o'clock. At 7 h. A.M. the rods being lifted from the grass, it was perceived, that the under sides were perfectly dry, while all the rest was quite wet with the dew that had fallen. The fourteen stands, comprehended between the first and sixteenth, having their distances gradually reduced from twenty feet three

inches to twenty feet, the operation of re-meaſurement was then begun, by placing the rods in coincidence with each other (which was now found to be eaſily and accurately effected by a few repeated ſtrokes with a wooden wedge only) until the fifteen rod lengths were meaſured off, and a fine line, corresponding with the ivory on the fifteenth, was drawn on the braſs ſlide. This line was found to be 0.$\frac{4\,9\,8}{1\,0\,0\,0}$, or near half an inch beyond that which terminated the 300 feet the preceding evening. Hence it is evident, that the dew imbibed only in one night, or a ſpace of time not exceeding fourteen hours, occaſioned ſuch an expanſion in the deal rods, as in the whole baſe would have amounted to 45.484 inches.

It is ſufficiently obvious, that this laſt mentioned experiment was more accurate, in the proportion of about fifteen to one, than any compariſon we could at that time have made with the ſtandard. But ſince immediately after it was finiſhed, the ſun ſhone out very bright, it is by no means certain, how ſoon the rods would again have contracted to their former length, or near it, had they been expoſed to his rays. Repeated compariſons for aſcertaining facts of this ſort, at very ſhort interims, are abſolutely incompatible with the nature of ſuch tedious and troubleſome operations as the meaſurement of long baſes: and here, indeed, lies the great objection to the uſe of deal rods, that at no time can we be certain how ſoon, after a compariſon has been made, they may alter their length in a proportion, and ſometimes too even in a ſenſe, different from what was expected.

Deſcription

Deſcription of the Glaſs Rods, ultimately made uſe of to determine the length of the Baſe. Tab. XIX.

It has been already mentioned, that the week of rainy weather in the end of July was employed in providing the glaſs tubes, and in concerting matters with Mr. RAMSDEN, relative to their conſtruction as meaſuring rods. Notwithſtanding their great length, they were found to be ſo ſtraight that, when laid on a table, the eye, placed at one end looking through them, could ſee any ſmall object in the axis of the bore at the other end.

The nature and conſtruction of the glaſs rods, whereof three were finiſhed for the operation, will be beſt conceived by conſidering, with care and attention, the plans and elevations of them, in whole or in part, to different ſcales in tab. XIX.; where likewiſe may be ſeen, plans and ſections of the ends of the tubes, in their real dimenſions, for the better underſtanding the ſeveral parts of the apparatus placed therein.

The caſe containing the tube, and which ſerves to keep it from bending in its original ſtraight poſition, is every where of the depth of eight inches, of the ſame width in the middle, and tapers from thence, in a curvilinear manner, towards each end, where it is only two inches and a quarter broad. It is made of clean white deal, the two ſides being half an inch, and the top and bottom three-eighths in thickneſs. Theſe laſt are placed in grooves fitted to receive them, about half an inch from the upper and lower edges of the ſides, which bending eaſily, and applying cloſely, are then firmly faſtened by two rows of wood ſcrews on each ſide, to the top and bottom

reſpectively. Thus, the depth of the ſides in one ſenſe, and the ſpring which they have by bending in the other, act as truſſes, prevent the caſe from warping, and render it ſufficiently ſtrong, although at the ſame time, conſidering its great length, very light.

The plan of the middle rod repreſents the caſe with the top off, that the tube may be ſeen placed therein: the right and left-hand rods have the tops on, whereby may be ſeen the oval opening in the middle of each, ſhut by a mahogany lid; and alſo the poſitions of the two thermometers, with tubes bent at right-angles, ſo as to place the ball about two inches downwards within the caſe, for the better aſcertaining the temperature of the glaſs, as will eaſily be conceived, by conſidering the repreſentation of the tube and ball in the ſection acroſs the middle of the rod.

It is to be obſerved, that the middle of the tube is made faſt to the middle of the caſe in the following manner. Firſt, around the middle of the tube, a quantity of pack-thread, immerſed in liquid glue, was wound by ſeveral returns on itſelf, for the ſpace of about two inches in length; and upon this maſs of pack-thread, while the glue was warm, a ſtrong mahogany collar was forced; whereby the three ſubſtances became ſo perfectly united to each other, that they might be conſidered as one only. Acroſs the bottom of the caſe in the inſide, three mahogany braces or girders, one in the middle, and one half-way between it and each end, are faſtened, by means of ſcrews, to the bottom and ſides. Theſe riſe about 1¼ inch above the bottom, ſo as to place the axis of the tube, when in uſe, about 2¼ inches above the ſurface of the ſtands on which it reſts. The end-pieces of the caſe are likewiſe of mahogany, about 1¼ inch thick. Each conſiſts of two parts, a lower and an upper.

upper. In the lower parts, as well as in the croſs braces, there are ſemi-circular cavities lined with broad-cloth, fitted to receive the diameter of the tube, which reſts in them, and is conſequently ſupported at five different points. The upper end-pieces, having likewiſe ſemi-circular cavities fitted to embrace the upper part of the tube, ſlip down upon it, when it has been, by repeated trials, brought to its true poſition; that is to ſay, the axis of the bore into the ſame ſtraight line, the caſe being all the while ſupported by its extremities ou two ſtands only, in the manner in which the rods are applied in actual meaſurement. The braces within the caſe have alſo their upper pieces, which, in like manner, apply cloſely to the tube, and are fixed to the lower ones by means of ſcrews. The whole together ſerve only as ſtays to keep the tube in its true place from ſhaking; but without binding it however too cloſely. Laſtly, the mahogany collar glued to the pack-thread on the middle of the tube, being ſtrongly fixed by four ſcrews to the middle brace, as may be ſeen in the ſection, is that by which the tube is kept perfectly immoveable with reſpect to the middle of the caſe; while it is unconfined longitudinally in the cavities lined with broad-cloth every where elſe.

Both ends of the tube are ground perfectly ſmooth, and truly at right-angles to the axis of the bore. That end, which in meaſuring uſually lies towards the left-hand (ſince moſt people will work the ſcrew with the right) projects about ſeven-tenths of an inch without the caſe, and is called the fixed end, becauſe the apparatus belonging to it is fixed. The other end towards the right-hand projects about nine-tenths of an inch, and, having a moveable apparatus, is called the moveable end.

The

The fixed apparatus conſiſts of a cork about three inches in length, made of the very beſt material, and ſo nicely fitted to the bore as juſt to admit of being forced into without burſting it. In the middle of the cork a cylindrical braſs tube is placed, whoſe ſides are thin, the inward end thick, and the outward end open. It receives a ſteel pin, whoſe inward end being formed into a ſcrew, is thereby fixed into the thick metal of the tube. The ſteel pin carries outwardly a button and neck of bell-metal. The neck fits ſo very cloſely the open end of the braſs tube as to prevent any ſhake there; at the ſame time that the inſide of the button applies very juſtly to the ground end of the glaſs tube, to which the outward ſurface (being a true plane) is exactly parallel.

The moveable apparatus conſiſts, like the other, of a cork and braſs tube of the ſame length. Before the inſertion of this cork, an oblong piece ſeven-tenths of an inch long, and two-tenths broad, was cut from it, in that part of its cylinder anſwering to the upper part of the outward end of the glaſs tube, on the inward ſurface of which, about half an inch from the end, a fine line had been previouſly cut by a diamond point. The braſs tube in this cork contains within it a looſe ſteel worm, or helical ſpring, ſomething leſs than the interior diameter of the tube. Along the cavity formed by the ſpiral, there paſſes a ſteel pin, like that in the fixed end; but it is longer, and has no ſcrew at the inward end, that being nicely ground, ſo as to fit a circular hole in the inward end of the braſs tube, while a triangular bell-metal neck fits one of that figure in the outward end. Thus the pin moves freely backwards or forwards without any ſhake, and preſſes upon the ſteel ſpring, by means of a circular braſs collar, placed for the purpoſe, at the inward end of the neck; while the outward

end is attached to a bell-metal button. The outward surface of this moveable button is spherical, described on a radius of about two inches; while the inward surface, like that at the fixed end, would apply closely to the ground end of the glass tube, but should not be pushed so far forward as to touch it. A circle and narrow slide, cut from a solid cylinder of ivory, fitted originally to enter easily the glass tube, is attached to the inside of the button by small screws, and permits the neck to pass through a hole made on purpose in the circle. The slide is about eight-tenths of an inch long, and has a fine intersection cut upon it near the inward end, made black to render it more conspicuous. Thus, two rods being brought into contact, and the fixed button of one being prefled againſt the moveable button of the other, the intersection is thereby pushed forwards until it coincides with the diamond line on the interior surface of the tube; whose length is so adjusted, as that, when the coincidence is perfect, the distance between the plane surface of one button, and the spherical surface of the other, is exactly twenty feet. The left-hand side of the plate represents the relative positions of the extremities of the first and second rods, when the ivory is in coincidence with the diamond line. And the right-hand side shews the relative situations of the extremities of the second and third rods, before the ivory is brought to coincidence with the diamond line, the slide being then pushed out by the action of the spiral spring within the cork.

Every rod has four wheels, two at each end. They are two inches in diameter, and connected by a common steel axis, which rises and falls in a vacuity prepared for its admission in the mahogany end-pieces, the under part of which vacuity is afterwards filled up.

A brass

A braſs ſtrap or bridle, about eight-tenths of an inch broad, paſſes over the top of the caſe, and deſcending down each ſide, bends outwards, ſo as to form a projection for the reception of the wheels, whoſe pivots turn in, but near to the lower end of the bridle, which is kept in its place by means of the two ſide ſcrews working in grooves, and the milled-headed ſcrew at top. This laſt ſerves likewiſe to raiſe or depreſs the wheels at pleaſure.

Each rod has two croſs feet, placed immediately behind their reſpective pair of wheels, extending outwards about 4¼ inches from the center on each ſide. Under their outward extremities, ſmall pieces of hardened ſteel, formed into the teeth of a file, are fixed by means of ſcrews. When the firſt rod has been laid in its true place, by unſcrewing the milled heads, the wheels are ſuffered to riſe; whereby the whole weight is removed from them, and thrown upon the teeth of the files, which then indent themſelves into the ſurface of the ſtand, and become as it were united to it. But when the fixed button of the ſecond rod is brought to preſs againſt the moveable button of the firſt, the weight being then thrown upon the wheels by ſcrewing the milled heads at top, the rod is eaſily moved on by the following apparatus.

The three rods are numbered, as were thoſe of deal, 1.2; 3.4; 5.6. On the firſt or odd end of each rod 1. 3. and 5. there ſtands a braſs fork, about two inches high, fixed by four ſcrews and an oblong plate to the top of the caſe. On the ſecond, or even end of each, 2. 4. and 6. there ſtands a braſs pillar of the ſame height with the fork, likewiſe fixed to the top of the caſe by four ſcrews and a circular plate. Two ſteels rods or hooks were indifferently uſed for bringing up the moveable rod (the weight then lying on the wheels) into its true place. They

are

are both represented in the plate, and only differ from each other in the shape of the brass milled-headed nuts that work upon the screw, of about 2¼ inches in length, into which the right-hand end of each hook is formed. Thus, while the nut enters very freely into, and rests upon, the fork, the left-hand end of the hook has a circular hole in it, whereby it slips easily off and on of the brass pillar. By referring to the plate, it will appear sufficiently obvious, from the nature of the nut on the left-hand hook, that it could only move the rod on to coincidence, and could not bring it back again, if the business happened at any time to be overdone; in which case it was necessary to move the rod a little backwards by the hand, and then to work anew with the nut, until the coincidence was accurate: whereas the nut on the right-hand hook, having two shoulders, could either push or pull the rod forwards or backwards: and although this appeared to be an advantage, yet it was found from experience, that it rather bound the hook too much, and occasioned a kind of spring in the parts, which sometimes disturbed the coincidence on the removal of the hook; wherefore it was often applied, like the other, by placing the screw itself in the fork, and working with both shoulders of the nut behind it.

The positions of the thermometers, and mahogany oval lid on the top of the case, have already been mentioned. This last, being unlocked and removed, permits the case to be looked into, or the hand to be admitted, in order to be certain that the fastenings remain safe and entire in the inside. Brass caps, with the respective number of the rods engraved on them, are likewise screwed on the male-screws in the ends of the case, through which the extremities of the tubes project, to preserve them from accidents when not in use. And, lastly, to

ſtrengthen the caſes, but more particularly to prevent them from being rent when long expoſed to the ſun's rays in the field, the ſides are covered with brown linen laid on very ſmoothly, and carefully glued with thin glue, uſed as a ſtronger kind of paſte, to which it may yet be neceſſary to add a coat of oil paint.

Each of the glaſs rods, completed in the manner abovementioned, weighs about ſixty-one pounds. Their lengths were aſcertained by means of new braſs points placed in the great plank, the ſpaces of forty inches being laid off, with the utmoſt care, from the braſs ſcale, when the temperature of all had remained for the greater part of two days (Auguſt 15th and 16th) at or very near 68°. For this purpoſe two braſs rectangular cocks, whoſe alternate ſurfaces had been previouſly ground together, were placed upon the plank, ſo as to biſect the extreme dots; in which ſituation they preſented to each other ſurfaces that were truly parallel. The rods being then ſeverally placed between the cocks * (or, as was found to be a better method,

* The firſt of theſe cocks, or that to which the fixed button was applied, had a hole in it exactly of the height of the center of the button, and large enough to permit the point of the micrometer ſcrew to paſs through it, the ſaid ſcrew being fixed on the farther ſide, or beyond the cock. Thus, while the temperature continued accurately at 68°, the fixed button, or any other plane ſurface, being brought up to the hole in the cock, and the micrometer point ſcrewed ſo far as juſt to touch it, the coincidence continuing in the interim perfect, the exact diſtance of twenty feet was obtained between the point of the ſcrew and the ſecond cock; at which time the diviſion anſwering to the index on the head of the micrometer was carefully noted. This being done, the cock with the hole was removed from the plank, and the rods were ſeverally adjuſted by being placed between the point of the ſcrew and the ſecond cock. This ſubſtitution of the micrometer point, inſtead of the firſt cock, was found neceſſary; becauſe, during the operation of adjuſtment, the temperature would ſometimes change a degree, generally

method, between the point of a micrometer screw, supplying the place of the first cock, and the second) the ivory intersection was at first necessarily carried beyond the diamond line, so as to make the intermediate space less than it should be, until by the gradual grinding down of the moveable bell-metal button, it was enlarged to twenty feet, as then shewn by the accurate coincidence of the intersection with the diamond line.

It was by these distances in the great plank, prolonged to twenty-five feet, that the new length of the steel chain was now settled, so as to obtain the full one hundred feet at four measurements. At this time too, brass points were introduced into the chain at every twenty-five feet, whereby its extent may be compared on any future occasion; but the temperature had now fallen to 66°½.

Disposition of the Stands for the double measurement with the Chain and Glass Rods; description of the apparatus then applied to the ends of the Chain; and ultimate continuation of the measurement with the Glass Rods alone. Tab. XVII. and XIX.

From the various circumstances already mentioned, in the course of this tedious, yet necessary recital, it had been for a considerable space of time foreseen, that the result given by the measurement with the deal rods must be entirely rejected,

generally in excess, from handling the instruments. One degree of alteration, producing a difference of about $\frac{1}{1000}$th part of an inch in the twenty feet, was very easily and accurately allowed for by such a micrometer as this, which shewed the coincidence of the ivory intersection with the diamond line to be more or less perfect, when the head of the screw was moved two divisions, that is to say, $\frac{1}{2000}$th or $\frac{1}{3000}$th part of an inch.

and that by the glaſs rods adhered to, as every way deſerving of the preference; becauſe of the obvious impropriety there would be, in taking a mean between one indiſputably good and another leſs perfect, however ſmall or trifling in reality the difference of the two might ultimately be found, on a minute and ſcrupulous compariſon.

In order, therefore, to avoid any repetition of the operation with the glaſs rods, and at the ſame time to give ſomething like a fair trial to the chain, it was propoſed, that a double meaſurement ſhould be carried on with both at once; that is to ſay, that the number of ſtands, and ſeveral other parts of the apparatus, ſhould be ſo far augmented, as to admit the chain to be placed twice in advance, and then the rods to follow in ſucceſſion on the ſame ſtands. Accordingly, the various articles having been ſent to the north-weſt end of the baſe on the evening of the 17th of Auguſt, the operation of the double meaſurement commenced next morning the 18th.

By referring to tab. XVII. it will be ſeen, that ſeventeen ſtands were neceſſary for ſupporting the chain, the apparatus attached to each end of it, and ten coffers, whereof every five made about ninety-eight feet, in order that one length of the chain being meaſured off in the firſt five, it might be drawn forward into the laſt five, and ſo on. Theſe ſeventeen ſtands were diſpoſed of in three groups of three each, and four intermediate, between the central and extreme groups. The middle or ſlide ſtand of each group (ſo diſtinguiſhed becauſe ſome of them had braſs ſlides on their tops) ſupported the handle of the chain, and of courſe received the traces made at the feather-edged pieces of braſs, terminating the beginning and ending of the hundred feet. Thus, there were in all ſix ſtands, intermediate to thoſe in the center of each group that ſupported

the ninety-eight feet of coffering, which was kept ſo much ſhort of the hundred feet, that its extreme parts might not reſt upon, or even touch, the central ſtands. To that on the left of the center was attached the apparatus for the firſt or zero end of the chain; and to that on the right of the center was attached the apparatus for the laſt end of the chain. When the ſecond chain length had been meaſured off, the firſt and ſixth of the coffer ſtands of the firſt chain were moved forward to prepare for the third chain; and the four remaining coffer ſtands were raiſed, until their ſurfaces came into the ſame plane with the ſlide ſtands, for the reception of the glaſs rods. The ſpace by which theſe ſtands were raiſed was about three inches; for ſo much higher was the ſurface of the interſole or flooring of the coffers than the ſtands which ſupported them.

The apparatus attached to the firſt end of the chain, or that which ſerved to pull it back to the point of commencement, while a weight continued ſuſpended at the farther end, conſiſts of two parts, as may be ſeen by referring to the left-hand ſide of tab. XVII. Firſt, a ſmall wooden frame, fitted to ſlip on to the top of any one of the ordinary ſtands, placed immediately to the left of that which ſupports the handle. Secondly, a flat ſteel rod, about two feet in length, wherein a number of holes are pierced, about an inch aſunder, for the reception of a ſteel pin placed in one of the holes, as beſt ſuits the diſtance of the ſtand from the handle. That end of the ſteel rod neareſt to the end of the chain is formed into a ſcrew about four inches in length, and it receives upon it a forked hook fitted to lay hold of the ſtraight part of the handle of the chain. Within the forked hook there works a ſtrong milled-headed braſs nut, which acting upon the bottom of the fork, the chain is thereby pulled back, until the wire ſuſpending the plummet

from

from the dart on the feather-edge coincides with the point of commencement on the ground underneath; for which purpose there is a hole in the top of the ſtand through which the wire paſſes. The apparatus ſtand, thus ſerving to pull back the chain, was commonly loaded with double weights, placed on the two hindermoſt legs.

The apparatus for the laſt end of the chain conſiſts, like the former, of a ſmall wooden frame that can be readily ſlipped upon any of the common ſtands, as may be ſeen by referring to the right-hand ſide of tab. XVII. This frame carries a pulley, over which a rope paſſes having fourteen pounds weight ſuſpended at one end of it, while a forked iron hook at the other end lays hold of the ſtraight part of the braſs handle. By means of theſe two apparatuſes the chain is always kept to the ſame degree of tenſion in its coffers, in each of which a thermometer was placed to indicate the temperature; the whole being covered up from the direct rays of the ſun by a narrow piece of linen cloth, ſtretched along it from one end to the other.

Each coffer conſiſted of three boards about half an inch thick. The ſides were about five inches deep, nailed at the middle to an interſole bottom of four inches, in ſuch manner as to be repreſented in ſection by the letter H. They were ill made, being by their parallelogram ſhape apt to warp, which might have been prevented by giving them the figure of the caſes of the glaſs rods, that is to ſay, making them wide in the middle and narrow at each end.

We are now to proceed to give ſome account of the double meaſurement with the chain and glaſs rods; wherein it muſt be remembered, as alſo in continuing the operation with the glaſs rods alone, that in referring to the map for the daily progreſs in the work, we are going from the forty-ſixth towards the firſt ſtation;

ſtation; and in having recourſe to the general table of the baſe, for altitude, temperature, or correction for expanſion, we are aſcending from the bottom towards the top, contrarily to the order in which the operation with the deal rods was conducted.

On the morning of the 18th of Auguſt, the ſtands with the various parts of the apparatus being placed in the manner juſt now deſcribed, the operation was begun by bringing the firſt end of the chain to coincide with the interſection on the tripod, anſwering to the end of the 1370th deal rod, and 4.31 feet diſtant from the center of the pipe terminating the north-weſt extremity of the baſe. The chain being ſtretched along its five coffers by the fourteen pounds weight ſuſpended over the pulley at the farther end, and the temperatures of the five thermometers being regiſtered in a book kept for that purpoſe, a fine trace was made on a piece of card faſtened under the feather-edge at the farther handle, denoting the end of the firſt hundred feet. The chain being then moved on into the next five coffers, thoſe that had been thus vacated were carried forward to prepare for the third chain length, and thereby permit the firſt ſet of ſtands to be elevated for the reception of the glaſs rods; and ſo in ſucceſſion with the others.

In this manner we proceeded, and in the courſe of the day were only able to meaſure the length of ten chains, or 1000 feet, being the forty-ſixth and forty-fifth hypothenuſes of the baſe, the firſt of 400 and the laſt of 600 feet. Being arrived at this point it was found, that the fine line on the braſs ſlide, marking the extremity of the tenth chain, fell ſhort of another fine line on the ſame ſlide, denoting the end of the fiftieth glaſs rod, juſt two-tenths of an inch. Now it will appear hereafter, when we come to ſhew, by the experiments with the pyrometer, what the real contractions of the chain and

glaſs

glaſs rods were, for the degrees of difference of temperature * below that in which their reſpective lengths were laid off, that this ſmall apparent difference of two-tenths of an inch, between the two modes of meaſuring the thouſand feet, ſhould have been 0.17938 in. to have made the two reſults exactly agree, which is a real difference of only 0.02062 of an inch. Suppoſing then every thouſand feet of the baſe to have been meaſured by the chain with the ſame attention, and conſequently with the ſame, or nearly the ſame ſucceſs (and there ſurely cannot be any reaſon to doubt of the practicability) we ſhall have 27.404 × 0.02062 in. = 0.565 in. or a defect of ſomething more than half an inch on the whole length of the baſe.

* When the length of the chain was laid off, the heat was 66°¼, and that of the glaſs rods 68°. They will, therefore, only agree with each other accurately in theſe reſpective temperatures. The mean of twenty thermometers for the four chain lengths of the forty-ſixth hypothenuſe gave a heat of 61°.6; and for the ſix chain lengths of the forty-fifth, the mean of thirty thermometers gave 59°.75. The temperature of the 400 feet of glaſs by the mean of forty thermometers was 65°.3; and of the 600 feet, by the mean of ſixty thermometers, it was 60°.8. Now, from theſe data, and the expanſions of ſteel and glaſs, as determined by the pyrometer, the computation will ſtand as follows:

				In.	In.	In.	
Steel	400	66.5 − 61.6	= 4.9	× 0.03032 = 0.14955	= 0.45856		contract. of 1000 feet.
	600	66.5 − 59.75	= 6.75	× 0.04578 = 0.30901			
Glaſs	400	68.0 − 65.3	= 2.7	× 0.02068 = 0.05584	= 0.27918		contract. of 1000 feet.
	600	68.0 − 60.8	= 7.2	× 0.03102 = 0.22334			
The 1000 feet of ſteel ſhould have contracted more than the 1000 feet of glaſs,					= 0.17938		
But the difference was found to be					= 0.20000		

Therefore the error of the chain in defect was 0.02062 × 27.404 = 0.565 in. or little more than half an inch on the whole baſe.

So

So nice an agreement between two reſults, with inſtruments ſo very different, could not fail to be conſidered as aſtoniſhing; and as it rarely happens, that the graduation of thermometers will ſo nearly correſpond with each other, as not to occaſion a much greater error, all were very deſirous that it could have been farther confirmed by continuing the operation in the ſame way through a more conſiderable proportion of the whole length. But beſides the tedious nature of the double meaſurement, owing to the multiplicity of ſtands, platforms, coffers, and other articles, that were now ſucceſſively to be moved forward, and for which purpoſe it had been found neceſſary to re-inforce the party of ſoldiers with ſix additional men; the operation had already trained out to a much more conſiderable length than had been expected; the ſummer was now far advanced, and the continuance of good weather uncertain; the coffers likewiſe for the chain, having been conſtructed in a hurry, were found to be defective: in ſhort, all theſe reaſons contributed to induce us to give up, for the preſent, any farther experiment with the chain, and to proceed with the glaſs rods alone in the completion of the meaſurement.

Accordingly, on Thurſday the 19th of Auguſt, the operation with the glaſs rods was continued for the five hypothenuſes, from the forty-fourth to the fortieth inclufive. It will be remembered, that in proceeding with the deal rods, double pickets had been placed in the ground, at the middle of the forty-firſt hypothenuſe, or that point which terminated the 1215th rod, reckoning from the ſouth-eaſt, or the 155th from the north-weſt end of the baſe. Now, in returning to this point with the glaſs rods, the extremity of the 155th fell ſhort of the ſilk thread ſtretched from picket to picket, juſt one-tenth of an inch. The expanſion of the braſs ſtandard ſcale, and

that of glaſs being taken into the account, it appears, that the ſmall expanſion * of the deal rods from the humidity of the air, muſt, at this point, have exceeded what it was eſtimated at in the general table by 0.931 of an inch, ſuppoſing no error of any kind whatever to have ariſen in the execution, from bringing the rods into contact, or otherwiſe.

On Saturday the 21ſt of Auguſt, the meaſurement was reſumed at the thirty-ninth ſtation, and continued for five hypothenuſes to the thirty-fifth incluſive.

This day, about noon, HIS MAJESTY deigned to honour the operation by HIS preſence, for the ſpace of two hours, entering very minutely into the mode of conducting it, which met with HIS gracious approbation.

On Monday the 23d, the menſuration was farther continued for five hypothenuſes, that is, to the thirtieth incluſive.

On Tueſday the 24th, we proceeded with the meaſurement for the ſpace of ſeven hypothenuſes, finiſhing the buſineſs of the day at the twenty-ſecond ſtation.

	In.	
* 155 deal rods = 3100 feet	+0.383	for 1° exceſs of temperature of the braſs ſcale from 62° to 63°.
	+0.651	proportionable part of the eſtimated expanſion from humidity.
	+1.034	equation of the deal rods on 3100 feet.
155 glaſs rods = 3100 feet	+2.301	for 6° exceſs of the heat of the braſs ſcale from 62° to 68°.
	−0.436	obſerved contraction of the glaſs from the 11th and 12th columns of the table.
	+0.100	by which the 155th rod fell ſhort of the thread.
	+1.965	equation of the glaſs rods on 3100 feet.
0.931		Difference of the two equations, under-rated in the expanſion of the deal rods.

It

It will be remembered, that in carrying on the operation with the deal rods, double pickets were left in the ground at the twenty-ſeventh ſtation, anſwering to the extremity of the 810th rod from the firſt, or the 560th from the laſt end of the baſe. Now, on arrival at this point, the 560th glaſs rod overſhot the ſilk thread, ſtretched from one picket to the other, 2.525 inches. Here again we find, that the lengthening * of the deal rods from the moiſture of the atmoſphere differs but little from what it has been eſtimated at by compariſon with the ſtandard, being over-rated only two-tenths of an inch on the 560 rods. In this day's operation, in paſſing the bridge laid over the old river, the meaſurement, inſtead of being made in the hypothenuſal, was carried on in the level line, for the ſpace of twenty rods, namely, fifteen rods of the twenty-ſeventh, and five of the twenty-ſixth hypothenuſe; which occaſions the alteration in the reduction of theſe two ſpaces, marked with aſteriſks in the general table.

As ſome trouble had been found to attend the croſſing of the great road, in the firſt meaſurement, owing to the number of carriages that were continually paſſing, the depth of

		In.	
* 560 deal rods = 11200 ft.	+	1.390	for 1° exceſs of heat of the braſs ſcale from 62° to 63°.
	+	5.258	eſtimated expanſion from moiſture,
	+	6.648	equation of the 560 deal rods.
560 glaſs rods = 11200 ft.	+	8.343	for 6° exceſs of heat of the braſs ſcale from 62° to 68°.
	+	1.821	obſerved expanſion of glaſs } from columns 11th
	−	1.191	obſerved contraction of ditto } and 12th.
	−	2.525	over-ſhot the ſilk-thread.
	+	6.448	equation of the 560 glaſs rods.
		0.200	Difference over-rated in the expanſion of the 560 deal rods.

the ditches, and height of the banks of the old Roman way; therefore treſtels, ſuited for the purpoſe, had been now prepared; and leſt any accident might have happened in conducting this part of the operation, ſo as to oblige us to a repetition, double pickets were placed in the uſual manner in the ground two rod lengths from the twenty-ſixth ſtation, to which we could have referred, without going back as far as the tripod left at the twenty-ninth ſtation, the point from which we had departed in the morning.

Bad weather prevented any progreſs being made on the 25th; and, on the 26th, all that could be done was to meaſure the twenty-ſecond and twenty-firſt hypothenuſes.

On Friday the 27th, the work went on more expeditiouſly, having in the courſe of that day meaſured ſix hypothenuſes, and placed the tripod at the fourteenth ſtation.

On Saturday the 28th, eight hypothenuſes were meaſured, and the tripod was placed at the ſixth ſtation. In this day's operation, being arrived near the bridge laid over Wolſey River, double pickets were placed in the ground in the point anſwering to the extremity of the 1172d rod, reckoning from the north-weſt, or the 198th rod from the ſouth-eaſt end of the baſe, that we might recur to them in caſe of accident; and the eighteen rod lengths, between this point and the ſixth ſtation, were meaſured on the level, inſtead of the hypothenuſal line, which required the alteration of the reduction as diſtinguiſhed by the aſteriſk in the general table.

On Monday the 30th of Auguſt, the meaſurement with the glaſs rods was completed*; when the extremity of the 1370th rod

* The gentlemen who were preſent at, and aſſiſting in, the laſt day's operation were Captain Bisset, Mr. Greville, Sir William Hamilton, Mr. Lloyd, and

rod over-shot the center of the pipe terminating the base towards the south-east by 17.875 inches, or 1.49 foot. Hence, when the several equations for expansions are respectively taken into the account, we find, that the alteration of the deal rods from the humidity of the air, which, by comparison with the standard, was apparently most considerable in the first and second sections of the base, has now wholly vanished; that is to say, the total amount of it has been over-rated by 20.964 inches*; and this is the contradictory circumstance that has been formerly alluded to.

I have already suggested what appear to me to have been the only three possible causes of this difference, found between the estimated and real expansion of the deal rods; and as we are to abandon that measurement entirely, it is of little or no importance now to endeavour to discover, were it possible, whence it may have arisen. If any error was actually com-

and Dr. Usher, Professor of Astronomy in the College of Dublin. This last gentleman was so obliging as to observe, with the most scrupulous attention, throughout the whole operation with the glass rods, that the coincidence of the second with the first remained undisturbed, while that of the third with the second was completing.

* 1370 deal rods = 27400 ft.

	In.
+ 3.389 for 1° of the brass scale from 62° to 63°.	
+ 24.223 estimated expansion from humidity.	
+ 27.612 equation of the 1370 deal rods.	
+ 20.336 for 6° of the brass scale from 62° to 68°.	
+ 5.089 observed expansion of glass from columns 11th and 12th.	
— 1.802 observed contraction of ditto.	
— 17.875 space by which the 1370th rod over-shot the pipe.	
+ 6.648 equation of the 1370 glass rods.	
20.964 over-rated in the total expansion of the deal rods.	

1370 glass rods = 27400 ft.

mitted,

mitted, which is the leaſt of all probable, it could only have happened at the place of the tripod, by bringing a wrong point of the ſtem over it when the operation was reſumed. But it is well known, how much care and pains were taken to prevent any thing of that ſort. Indeed the hypothenuſal diſtances, as given by the chain, agreed ſo nearly among themſelves, that even a foot or ten inches would have made ſo remarkable a difference in the ſituation of the next picket as could not have paſſed unobſerved. Beſides, in returning with the glaſs rods, after paſſing the Staines Road, the meaſurement was gradually found (without any leap whatever) to over-ſhoot the pickets, and at laſt over-reached the ſouth-eaſt pipe by 17.875 inches. I am therefore inclined to believe, that the difference ariſes partly from what may have been loſt by conſtantly butting one rod againſt the other, whereby the end of the 1370th did not reach ſo near to the north-weſt pipe as it ought to, and would have done, if the rods had been applied to each other by coincident lines. It muſt, however, be confeſſed, that the near agreement between the glaſs and deal rods in the upper part of the heath ſeems not perfectly reconcileable to this ſuppoſition. Nevertheleſs, the deſcent being quickeſt, and the irregularities of the ſurface much more conſiderable in the lower than the upper part, might produce ſome effect in one which did not take place in the other. But the chief part of the difference I take to have proceeded from over-rated expanſion; that is to ſay, the rods, when brought into uſe, contracted ſooner than we imagined, and thereby gave a ſhorter meaſure than what was aſſignable to them from the mean of any two or more compariſons.

The

The laſt day of Auguſt was employed in diſcharging the party, and removing the various parts of the apparatus to Spring-Grove Houſe.

Deſcription of the Microſcopic Pyrometer, made uſe of for determining by experiment the expanſion of the metals concerned in the meaſurement of the Baſe. Tab. XX.

Having, in the preceding part of this Paper, given a very minute account of the actual operations in the field, that the Public, being thus informed of every circumſtance, might be the better enabled to judge of the accuracy of the reſult, it remains yet to point out, in what manner the equations for the expanſions of the ſtandard ſcale, ſteel chain, and glaſs rods, applied to the apparent meaſurement of the baſe, in ſeveral of the preceding notes, have been obtained by means of experiments with the pyrometer.

It is ſufficiently well known, that many years ago, a very ingenious and valuable Member of this Society did publiſh in the Philoſophical Tranſactions (vol. XLVIII. 1754, N° 79.) an account of experiments made with a pyrometer of his invention. No doubt was entertained of the accuracy of the experiments here alluded to; on the contrary, they will be confirmed by the account now to be given of theſe recently made, with which they very nearly agree. But as different pieces of metal of the ſame kind are certainly ſuſceptible of different degrees of expanſion, it was judged beſt, on the preſent occaſion, to put rods to the teſt of thoſe very metals that had been made uſe of in the actual meaſurement of the baſe.

For, ſuppoſing both ſets of experiments to have been made with inſtruments equally perfect, and to have been in other reſpects equally well conducted, this muſt always be conſidered as the moſt unexceptionable method. Beſides, the expanſion of rods of the length of five feet being aſcertained, the unavoidable error of obſervations of this delicate nature, becomes leſſened in proportion to the exceſs of their length above ſhorter rods. In theſe new experiments too, another ſort of pyrometer, invented by Mr. RAMSDEN, has been applied, of ſuch accurate conſtruction that it ſeems not eaſy to improve it.

The microſcopic pyrometer, ſo named becauſe, by means of two microſcopes attached to it, the expanſion is meaſured, conſiſts of a ſtrong deal frame five feet in length, nearly twenty-eight inches broad, and about forty-two inches in height. The elevation of the eye-piece ſide, or that which preſents itſelf to the obſerver, and alſo of the micrometer end, or that which is towards his right-hand, as well as the general plan of the top, are repreſented by a ſcale of one inch to a foot, or one-twelfth part of the real dimenſions, in tab. XX. where likewiſe may be ſeen the angular view of the fixed end, together with plans, ſections, and elevations, of ſeveral of the principal parts, done to larger ſcales. From theſe, it is hoped, the conſtruction of the machine will be eaſily underſtood, without entering into a minute deſcription of the almoſt numberleſs ſmaller parts whereof it is compoſed.

On the top of the frame, two deal troughs, upwards of five feet in length, are firmly ſcrewed. That towards the obſerver overhangs the frame ſomething more than an inch; that on the farther ſide is even with the back part. Each of theſe troughs, which are about three inches ſquare in the inſide, contains a caſt-iron ſtandard prism, whoſe ſides are 1¼ inch.

The manner in which the priſms are faſtened to the bottoms of their reſpective troughs, and the nature of the apparatuſes they carry on their extremities, will be readily conceived, by referring to the particular plans and elevations of them, comprehended in the group of eight ſmall figures towards the right-hand of the general plan. Four of theſe appertain to the left-hand or fixed microſcope; and the other four to the right-hand or micrometer microſcope, ſo diſtinguiſhed becauſe it has a micrometer attached to it. By means of the braſs collars which embrace the priſms, their left-hand or fixed ends are ſcrewed down extremely faſt to the braſs pieces whereon they reſt, ſo as to be perfectly immoveable there with regard to their troughs; whereas their right-hand ends are kept eaſy, yet without ſhake, in their collars, that they may contract or lengthen freely as the temperature may require, without occaſioning any ſtrain upon the parts. The priſm in the neareſt trough may be called the eye-piece priſm, becauſe it carries the eye-pieces of the microſcopes; and that in the farther trough, the mark priſm, becauſe it carries the marks or croſs wires at which the microſcopes reſpectively point. The troughs are covered with pitch in the inſide, to make them hold water; and each has a cock in the left-hand end for diſcharging it.

Between the two deal troughs, one of copper, as a boiler, is placed, ſomewhat ſhorter than the former, but ſtill upwards of five feet in length. It is about 21 inches broad, and 3½ in depth. The center of the boiler, or rather the center of the object lens which ſtands in it, as we ſhall have occaſion ſoon to point out, is diſtant from the croſs wires of the mark 5.81 inches; and from the wires of the micrometer attached to the correſponding eye-piece 20.33 inches. The boiler reſts on five ſmall rollers, one being fixed to each end of the frame, and

the other three to the braces which run acroſs it. This copper trough has likewiſe a cock in the left-hand end; and in the general plan a caſt iron priſm is repreſented in it; but this laſt carries no apparatus, as thoſe in the wooden troughs do, being exactly of the length of five feet, and only placed there as one of the rods whoſe expanſion was tried, and to ſhew that the machine was capable of receiving a rod of that weight and magnitude.

By referring to the general plan it will be ſeen, that twelve lamps are made uſe of to bring the water in the copper to boil. They ſtand on four ſhelves, three in each compartment formed by the croſs braces of the frame. They can readily be puſhed forwards or drawn backwards, and when actually in uſe, their handles are only ſeen, projecting from under the copper. It was found, by burning oil in the lamps, the heat of the water could not be raiſed above 209° or 210°; but with ſpirits of wine it was brought into violent ebullition. The plan of the frame likewiſe ſhews, that the tubes of the microſcopes are ſub-divided into ſeveral diſtinct parts; and that one of theſe parts is attached by a collar to a mahogany priſm, which reaches from one end to the other. But the uſe of theſe contrivances it will be beſt to defer ſpeaking of, till after having deſcribed the apparatuſes that are placed within the copper boiler.

At the bottom of the plate the boiler is repreſented, both in plan and longitudinal ſection, to a ſcale of one-fourth part of its real dimenſions. It contains within it two braſs ſlides, the one long and the other ſhort; which, from the braces that bind the cheeks together, very much reſemble the form of a ladder. The long ſlide, whoſe cheeks are 1¼ inch deep, reaches almoſt the whole length of the copper, although every where unconnected with it except at the points A and B. At

the first of these, two strong pieces of brass, fixed to the cheeks, and notched underneath, embrace the ends of a brass cylindrical bar fastened to the bottom. At the last, the cheeks of the slide rest on a roller. Whence it follows, that the copper and slide remain immoveable with regard to each other at A; but from thence, towards either end, they have full liberty to change place; that is to say, to expand by heat, or contract by cold, in any proportion their different natures may require. The left-hand end of the slide is shut up by a strong perpendicular piece of brass, connected with the two side rings which support the object lens of the fixed microscope, whose center corresponds accurately with its inward face. This piece being firmly screwed to the cheeks of the slide, and counter-arched outwardly, forms a strong butt for the fixed end of the expanding rod (supposed here to be the steel bar) to act against. Within the right-hand end of the long slide, rests a short one of about 14¾ inches in length, whose cheeks are 1¼ inch deep. Its outward end, at C, rests on the cylindrical surface of the last brace of the long slide, fitted purposely to receive it; while a narrow longitudinal bar fixed in its inward end, at DE in the section, moves freely in the notch of a bridge F, framed for it in the long slide. The outward end of this short slide is shut up in a similar manner with the opposite end of the long one.

This end-piece is also connected with the two side rings which support the tube containing the object lens of the micrometer microscope, whose center is perpendicularly over its inward face, and being fortified outwardly by an edge bar, it forms a butt for the expanding end of the rod that is in experiment to push against. By attending to the plate it will be perceived, that to this end of the boiler a brass tube (R) is fixed, which contains within it a brass rod, surrounded by a helical

helical steel spring; which acting upon a broad shoulder of the rod prepared for the purpose, thereby presses its inward end, which enters the boiler, against the perpendicular surface of the end-piece of the short slide. Thus, the farther end of the rod in experiment, supposed now to be in its contracted state, is constantly made to bear against the surface that is under the fixed microscope. But on the application of heat, the irresistible force of expansion in the rod obliges the spring to give way; the short slide changes its place, and with it the object-lens of the micrometer microscope moves on a space proportionable to the degree of heat that is applied; and it is this distance, measured by means of the micrometer, as hereafter will be shewn, that determines the quantity of expansion, or the space by which the rod has lengthened. From the plate it will be further observed, that the rod in experiment rests on the surfaces of three rollers, about an inch in diameter; and by means of three pair of milled-headed nuts 1¼ inch in diameter, which move on axes that are formed into screws, until they almost touch the sides of the rod, this is kept in its true centrical position, whatever may be its form or lateral dimensions.

The microscope towards the left-hand has been denominated fixed, because it corresponds with the first or fixed end of the rod in experiment, and never changes its place while these are of the length of five feet. But it appearing to be of consequence, that the expansion of the standard brass scale, which is not quite forty-three inches long, should be determined, the pyrometer has therefore been adapted for the reception of any rods less than five feet, whereby it is made more universally useful. For this purpose it becomes necessary to move the marks and eye-pieces of the fixed microscopes along their respective prisms,

to

to the proper position for the rod that is to be tried. Nevertheless the object lens remains in its original place, and in its stead another lens, of the same focal distance, is fixed on a similar end-piece, that can be firmly clamped to any corresponding place whatever of the cheeks of the long slide. Hence will appear the reason for breaking the screening tubes of the microscopes into several parts, and the use of the mahogany prism, along which the thick part of the tube moves from one end to the other.

The pyrometer, since it was first made and tried, has undergone several small alterations, by way of improvements, which it is now unnecessary to describe particularly. One of these was the application of cross levels to the parts of the tube (8S in the general plan) connected with the object glasses. The manner in which they are fixed on will appear from the representations of them in the lowermost left-hand angle of the plate. And the section at the right-hand angle shews the appearance of the double brass hook, universal joint, and milled-headed nut, applied across the middle of the boiler (at TU) whereby the levels are brought to be consistent, when the water is boiling, with the position they had been adjusted to when the temperature was at freezing; that is to say, they are kept parallel to themselves in both states. This was thought necessary, because the application of the boiling water sunk the middle of the slide a small matter, and thereby made the levels run outwards.

The micrometer so often mentioned, being a very essential part of the machine, is represented both in elevation and horizontal section to the full size. Its chief parts consist of a micrometer steel screw, which works in the square nut of a brass slide, while the plain part of it enters into a long brass socket, nicely

nicely ground to receive it, and thereby preventing all ſhake. To the ſquare nut, one end of a watch chain is attached; the other end having paſſed around is fixed to a barrel, which contains a watch ſpring coiled up in the uſual manner. By this contrivance, any loſs of time in the motion of the moveable wire, fixed to the ſquare ſlide, is effectually prevented, whether the ſcrew be turned backwards or forwards. The fixed wire, ſo called becauſe it is only made uſe of occaſionally, appears in the elevation to the left-hand of the former, and is farther removed from the obſerver, being attached to the oval ſlide which bounds the field of the micrometer. This wire is moved by the inſertion of a milled-headed key (although not repreſented in the plate) fitted to ſlip upon the ſquare end of its proper ſcrew, which may be ſeen, in the elevation, projecting above the micrometer head. It has but little motion, being only intended for the meaſurement of ſmall differences of expanſion, or any ſmall ſpace, by leaving it there, while the other wire is repeatedly brought to coincide with, and again depart from it. For particular purpoſes this wire may be uſeful; nevertheleſs, the inſtrument would have performed very well without it.

The conſtruction of the microſcopes will be readily underſtood, by referring to the figures under that head on the right-hand ſide of the plate; where the relative ſituations of the different eye-glaſſes, with regard to the wires or place of the magnified image, as well as to the eye, are truly repreſented in their real dimenſions; but the diſtances from theſe to the object-lenſes and marks reſpectively, are contracted or broken off, from want of ſufficient room to delineate them otherwiſe. To increaſe the angle of viſion in microſcopes, it is always neceſſary that they ſhould have at leaſt two eye-glaſſes, and the fixed microſcope in the plate ſhews them in their

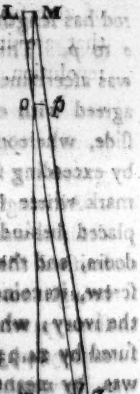
their usual position, the image from the object lens there being formed between the two, that the dispersion of rays in the first may be corrected by that of the second. But although this construction serves perfectly well every purpose of the fixed microscope, yet it could not answer in the moveable one, to which the micrometer is attached, where equal parts of an image, or their motion, are to be measured by the equable motion of the object lens, as shewn by the micrometer: for in that case, the interposition of an eye-glass before the image was formed, would not only have diminished its size, and thereby rendered the measure less accurate; but likewise, by refracting the oblique pencils more than those nearer the center, it would have destroyed the equality of the scale, and made equal parts of the object itself to have been represented unequally in the magnified image, and consequently erroneously measured by unequal parts of the micrometer. It was to remedy a defect of this sort that Mr. RAMSDEN proposed his new system of eye-glasses, described in the Philosophical Transactions, vol. LXXIII. 1783, N° 5. And he has here applied that system in the construction of the micrometer microscope; where it will be perceived, that both glasses stand between the eye and the image, whereby the greater magnitude of this last is obviously preserved, as well as the just similarity of all its parts to those of the object itself.

With regard to the scale of the pyrometer, it is, in the first place, to be observed, that the head of the micrometer screw, which is nine-tenths of an inch in diameter, is divided into fifty equal parts, each of which being reckoned two, it is therefore numbered to 100. Fifty-five revolutions of the head, being equal to 0.77175 of an inch, as measured with great accuracy by Mr. RAMSDEN's straight-line engine, it follows,

follows, that there are 71.27 threads of the ſcrew in an inch; that ſeven revolutions and nearly $\frac{1}{100}$th parts move the wire of the micrometer one-tenth of an inch; and that $\frac{1}{200}$th part of a revolution, or half a diviſion, anſwers to a motion of ſomething more than 0.00014 of an inch.

Having thus obtained the number of revolutions and parts of the micrometer (7.13) correſponding to one-tenth of an inch at the wires, it is ſufficiently obvious, that the number anſwering to one-tenth LM at the mark being likewiſe obtained, and added to the former, their ſum will give the meaſure of one-tenth at the object lens, or the ſpace by which the expanding rod has lengthened, as ſhewn by the motion of the lens from *o* to *p*. This meaſure of one-tenth of an inch at the mark was aſcertained in two different ways, and the reſults exactly agreed with each other. In the firſt place, a very thin ivory ſlide, whereon ſeveral twentieths of an inch were nicely divided by exceeding fine lines, was prepared, and made to move in the mark where the braſs ſlide now exiſts. A candle being then placed behind it at night, while the pyrometer ſtood within doors, and the micrometer wire being repeatedly moved by the ſcrew, its coincidence with the lines was diſtinctly ſeen through the ivory; whereby two of the ſpaces were found to be meaſured by 24.93 revolutions of the head. The ſecond method was, by means of two exceeding fine wires placed parallel to each other on the braſs ſlide, where they now remain, at the diſtance of one-twentieth of an inch on each ſide of the interſection wires, as may be ſeen by obſerving the real mark, or rather its magnified image, as ſhewn in the oval field of the micrometer, in the central figure of conſtruction. The revolutions of the micrometer anſwering to the diſtance between theſe parallel wires was, as before, found to be 24.93; which being

being added to 7.13, we have 32.06 for the number of revolutions measuring a motion of one-tenth at the object lens, or the expansion of one-tenth. In this manner Mr. RAMSDEN obtains the scale of his pyrometer in the easiest and most simple way imaginable, without any necessity for knowing the absolute distances of the object lens from the wires of the mark on one hand, and those of the micrometer on the other; distances not easily ascertained by actual measurement, on account of the position of that glass in its cell, which cannot conveniently be come at. Thus, in tab. XX. as well as in the annexed figure, LM being the object at the distance of the mark, equal to one-tenth of an inch; then *ml* will be its magnified image, in proportion to the former as *mo* is to *o*M. And, if through the point *p*, the place to which the object lens *o* has been carried by the motion of the expanding rod, a line M*q* be drawn parallel to L*l*, we shall have *ml* = 24.93 + *lq* = 7.13 = *mq* = 32.06, the number of revolutions of the micrometer measuring *op* the expansion. Having thus obtained the total number of revolutions corresponding to *mq*; and having likewise measured the total distance *m*M = 26.144 inches, a space easily ascertained between the wires of the micrometer and those of the mark, the partial distances *mo* and *o*M may then be readily found by computation: for *mq* : *ml* :: *m*M : *mo* = 20.33 inches; and *mq* : *m*M :: *op* : *o*M = 5.814 inches.



In order to finish the description of the pyrometer, it is only necessary to observe further, that the circular scale, seen in the elevation of the micrometer, whose head appears

pears to coincide with the dart on the plane part of the brass, is that which serves by its motion to register the turns of the head. A forked key, fitted to enter the holes near the circumference of the circle, is made use of for the adjustment of this zero. The circle should never be turned backwards or towards the left, lest the watch chain should thereby be thrown off the barrel, but always forwards or towards the right, even if it should be necessary to move it almost an entire revolution. The zero of the head is that which should be first brought to correspond with its proper dart. They may be seen to coincide in the horizontal section of the micrometer; and the departure of zero from this dart, indicates, by the number of divisions that are intercepted, the value of any fractional part of a revolution.

Account of the experiments with the Pyrometer.

Although the instrument which I have here endeavoured to describe was begun early in the winter of 1784, yet it was not finished till the beginning of last April; at which time it was brought to Argyll-Street, and being placed truly level on the stone pavement of the yard, was covered with an oil-cloth canopy, that the experiments might not be interrupted by rainy weather.

To fill the three troughs completely it required from twenty-five to thirty pounds of pounded ice, which was always put in with great care, so as to apply as compactly as possible to the standard prisms and rod respectively, with but little common water *

* When common water was used, although not in any very considerable proportion, the thermometer kept always half, and sometimes three quarters of a degree above 32°.

at

at first added; it having been found in these experiments, that ice water only, such as drains from the ice itself, is that which should properly be made use of to mix with the pounded ice, in order to bring the whole mass to the true freezing temperature. Being at the commencement uncertain what time might be necessary for the rods, especially when of so large a size as the standard prisms, to acquire the just temperature of freezing, at first the ice was put into the troughs over night, to prepare for the continuation of the experiment next morning. But after many repeated trials, this precaution was found to be needless; a quarter of an hour being more than sufficient to give to all the freezing temperature, as well as to render the lens on the expanding rod stationary, after the water supplying the place of the ice had been brought fairly to boil.

The instrument, in its first state, having in some cases made the expansion appear to be progressive, and not equable; therefore its rate was attempted to be ascertained by noting the progression answering to 60°, 120°, and 180° above freezing. But when the instrument was rendered perfect, and that no sensible difference was found between the expansion at the lower and that at the upper part of the scale, a fair mean being taken between its ascending and descending rates, and allowing for the difficulty of keeping the water, for any length of time, precisely to the same intermediate heat; then this tedious mode of conducting the experiments was given up, and the expansion for 180° was at once determined by bringing the water to boil around that rod, which but a little before had been dying in melting ice, and which the standard prisms still continued to do throughout each experiment, care being taken to have a supply of pounded ice always ready to keep these two troughs quite full.

Two obſervers are neceſſary for the effectual application of the pyrometer. He who obſerves with the fixed microſcope, takes care that its object lens is kept in its true place, that is to ſay, that the wire in the eye-piece accurately biſects the interſection wires of the mark. This he is enabled to do by means of the apparatus attached to the fixed end of the boiler, as will be beſt conceived by obſerving the plan (at WX) along with the elevation of that end placed near it. The apparatus conſiſts of two milled-headed ſcrews, working in braſs plates faſtened to the end of the frame, and acting againſt a ſmall cock which projects from the lower part of the boiler, whereby this laſt receives ſuch longitudinal motion to and fro on its rollers, as is ſufficient for the adjuſtment of the lens. He who obſerves with the micrometer microſcope, having brought the zero of the micrometer head to its dart, as ſhewn in the horizontal ſection, and alſo the revolution zero to its dart, as repreſented in the elevation, takes care, when the rod has acquired the freezing temperature, that the micrometer wire biſects the interſection wires of its proper mark. This he effects by working with the milled-headed ſcrew, repreſented in the plan and elevation of that mark, whereby the mark itſelf is moved until the biſection is accurate; and during the whole of this time, the firſt obſerver muſt be extremely attentive to keep his lens adjuſted.

One aſſiſtant at leaſt is neceſſary, who takes his ſtation on the oppoſite ſide of the pyrometer, to obſerve the levels, and keep them adjuſted, by means of the double hook applied near the middle of the boiler, and repreſented in the ſection on the line TU, at the lowermoſt right-hand angle of the plate.

The pyrometer having been adjuſted in the manner here deſcribed, by giving ſufficient time for the ſtandard priſms and

rod to contract to the true freezing temperature, as was eaſily known by the wires becoming perfectly fixed and ſtationary with regard to the marks; the ice was then removed from the copper trough; and the ſame being filled with water nearly on the boil, the ebullition was completed, and kept up, by means of the lamps now lighted for the purpoſe, and ſlipped in underneath.

The expanſion, anſwering to the 180° between freezing and boiling, was now meaſured by working with the micrometer ſcrew until the biſection * of its wire with thoſe of the mark was again complete; the obſerver at the fixed microſcope taking alſo eſpecial care all the while to keep his biſection perfectly accurate. The number of revolutions, regiſtered by the number of entire diviſions that the zero of the circular ſcale had departed from its dart or index, and alſo the value of any fractional revolution, regiſtered by the diviſions on the head intercepted between zero and its proper dart, were then noted, as expreſſed in the firſt column of the ſubjoined table of experiments; which requires no other explanation than what is therein inſerted, and which has been extended purpoſely to ſhew at one view, from inſpection only, how much the length of our baſe would have been affected, if meaſured by theſe metals reſpectively, in temperatures between 32° and 62°.

All the experiments were repeated at leaſt twice, and ſome of them three times, except the ſtandard ſcale and glaſs pen-

* This biſection of the wires may always be made to a great degree of preciſion, by one with a tolerably good eye, and accuſtomed to obſervations of this kind. I have myſelf repeatedly adjuſted the wires eight or ten times running, allowing another perſon to read off and unadjuſt each time, without the mean difference exceeding one-fourth of a diviſion of the head, which is only $\frac{1}{2500}$th part of an inch.

dulum

dulum rod, whoſe expanſions were only tried once. The difference of a few diviſions between the mean and extremes on the heat of 180°, being, in things of this ſort, of no importance, it was judged wholly unneceſſary to aim at a greater degree of preciſion in repeating them oftener. By referring to the table, particularly that column containing the expanſions on one foot by 180°, it will be perceived, that they are uniformly a ſmall matter leſs than what has been aſſigned to the ſame metals reſpectively, in the experiments formerly alluded to.

Ultimate determination of the length of the Baſe on Hounſlow-Heath.

In the former part of this paper, we have had occaſion to ſpeak of the ſeven firſt columns of the general table of the baſe; and the titles at the tops of the others reſpectively ſerve ſufficiently to explain thoſe towards the right-hand; the expanſion of glaſs above, and its contraction below 62°, contained in the eleventh and twelfth columns, being deduced from the recent experiments with the pyrometer.

	Feet.
The hypothenuſal length of the baſe, as meaſured by 1369.925521 glaſs rods of twenty feet each + 4.31 feet, being the diſtance between the laſt rod and the center of the north-weſt pipe, has been ſhewn to be	27402.8204
The reduction contained in the ſeventh column of the general table to be deducted is	0.0714
Hence the apparent length of the baſe, reduced to the level of the ſouth-eaſt extremity, becomes	27402.7490

The

	Feet.
The apparent length is to be augmented by the exceſs of the expanſion above the contraction of the glaſs rods, contained in the thirteenth column of the general table = 4.1867 inches, reduced to the heat of 62°, as has been uſually done in former operations of this nature	0.3489
The apparent length is farther to be augmented by the equation for 6° difference of temperature of the ſtandard braſs ſcale between 62° and 68°, this laſt being the heat in which the lengths of the glaſs rods were laid off = 20.3352 inches, as deduced from the experiments with the pyrometer	1.6946
Hence we have the correct length of the baſe in the temperature of 62° reduced to the level of the lowermoſt extremity near Hampton Poor-houſe,	27404.7925
This laſt length requires yet a ſmall reduction for the height of this lowermoſt end above the mean level of the ſea, ſuppoſed to be fifty-four feet, or nine fathoms, ...	0.0706
Hence the true or ultimate length of the baſe, reduced to the level of the ſea, and making a portion of the mean circumference of the earth, becomes	27404.7219

As ſome ſmall degree of uncertainty remains with regard to this laſt reduction, it may not be improper to ſay yet a few words on the principles that have been adhered to in making the computation. It will be remembered, that the meaſurement was made 31 feet above the ſurface of the heath, that being the height of the ſtands whereon the rods were placed; and

and that the teleſcopic ſpirit level gave a deſcent of 36.1 feet from the lowermoſt pipe to the ſurface of ſummer water in the Thames at Hampton. The accurate ſection of the river lately publiſhed, gives a fall of 13.33 feet from Hampton to the level of low water ſpring tides at Iſleworth. Now theſe three being added together, we have nearly fifty-three feet for the height of the baſe above Iſleworth. Having had no immediate means of determining what real difference there may be between Iſleworth and low water ſpring tides at the mouth of the Thames (for inſtance at the Hope or the Nore), I have ſuppoſed that fall to be about ſeven feet, ſo as to make the total deſcent ſixty feet. Now, ſuppoſing the ſpring tides at the Nore to riſe eighteen feet, if, according to M. DE LA LANDE's method, we deduct one-third of eighteen, *viz.* ſix feet from ſixty, we ſhall have fifty-four feet, or nine fathoms, that the mean ſurface of the ſea is below the meaſured baſe. Whether this concluſion be perfectly accurate or not is of no moment, ſince a whole fathom of difference (and I apprehend we are not farther from the truth) does not vary the reduction quite one-tenth of an inch. The reduced baſe has therefore been found by the following analogy: as the mean ſemi-diameter of the earth (ſuppoſed here to be 3492915 fathoms) augmented by nine fathoms, is to the mean ſemi-diameter, ſo is the meaſured baſe 27404.7925 to the reduced baſe 27404.7219 at the level of the ſea. It will doubtleſs be allowed, that infinite pains have been taken in the field and otherwiſe, throughout the whole of this operation, to obtain a juſt concluſion; but as the moſt accurate meaſurement imaginable is ſtill more liable to err in exceſs than in defect, we will throw away ſome uſeleſs decimals, and eſtabliſh the ultimate length of the baſe at 27404 feet and ſeven-tenths.

General

General Table of the Baſe, ſhewing the relative heights of the Stations above the ſouth-eaſt extremity near Hampton Poor-houſe, the Reduction of the Hypothenuſes, and the Correction for the Temperature of the Glaſs Rods; whence the true length is obtained in the heat of 62°.

1	2	3	4	5	6	7	8	9	10	11	12	13	14
Sections.	N° of hypothenuſes, 600feet each.	Relative heights.				Reduction of the hypothenuſes.	Number of rods, twenty feet each.	Temperature.		Correction for temperature.			Correct length of the baſe.
		Aſcent.	Deſcent.	Total aſcent.				Obſerved mean of 60 therm.	Exceſs or defect from 62°.	Expanſion above 62° + appt. leng.	Contract. below 62° — appt. leng.	Difference.	
				Hypoth.	Sections.								
		Feet.	Feet.	Feet.	Feet.	Feet.		°	°	Inches.	Inches.	Inches.	Feet.
First or ſouth-eaſt ſection, between Hampton Poor-houſe and Hanworth Summer-houſe.	1	0.07	— —	0.07		0.000012	29+	71 4	+ 9.4	+0.2909			
	2	1.855	— —	1.925		0.002875	.925521	80.7	+18.7	+0.5801			
	3	— —	1.855	0.07		0.002875	30	80.6	+18.6	+0.5770			
	4	2.745	— —	2.815		0.006288	30	74.0	+12.0	+0.3722			
	5	2.92	— —	5.735		0 007114	30	62 6	+ 0.6	+0.0186			
	6	0.76	— —	6.495		0.000489	30	58.7	— 3.3		−0.1024		
	7	1.57	— —	8.065		*0.000825	30	60.5	— 1.5		−0.0465		
	8	2.91	— —	10.975		0.007065	30	63.3	+ 1.3	+0.0403			
	9	— —	0.68	10.295		0.000393	30	64.1	+ 2.1	+0.0651			
	10	0.65	— —	10.945		0.000360	30	66.6	+ 4.6	+0.1427			
	11	— —	0.04	10.905		0.000009	30	70.4	+ 8.4	+0.2606			
	12	— —	1.18	9.725		0.001168	30	69 9	+ 7.9	+0.2451			
	13	0.83	— —	10.555	10.555	0.000581	30	62.7	+ 0.7	+0.0217			
Second or middle ſection, between Hanworth Summer-houſe and the north ſide of the Staines Road.	14	— —	0.94	9.615		0.000745	30	60.7	— 1.3		−0.0403		
	15	0.42	— —	10.035		0.000191	30	63.2	+ 1.2	+0.0372			
	16	— —	1.63	8.405		0.002222	30	69.0	+ 7.0	+0.2171			
	17	0.28	— —	8.685		0 000073	30	71.1	+ 9.1	+0.2823			
	18	4 16	— —	12.845		0.014439	30	68.6	+ 6.6	+0.2047			
	19	— —	0.44	12.405		0.000169	30	63.5	+ 1.5	+0.0465			
	20	0.19	— —	12.595		0 000036	30	59.2	— 2.8		−0.0869		
	21	1.87	— —	14 465		0.002922	30	56.2	— 5.8		−0.1799		
	22	0.73	— —	15.195		0.000452	30	57.0	— 5.0		−0.1551		
	23	0.39	— —	15 585		0.000135	30	64.1	+ 2.1	+0.0651			
	24	0.95	— —	16.535		0.000760	30	64.3	+ 2.3	+0.0713			
	25	0.49	— —	17.025		0·000208	30	62.5	+ 0.5	+0.0155			
	26	2.11	— —	19.135	8.58	*0.000063	30	71.1	+ 9.1	+0.2823			
Third or north-weſt ſection, between the north ſide of the Staines Road, and King's Arbour near the Colnbrook Road.	27	— —	0.71	18.425		*0.000214	30	70.5	+ 8.5	+0.2637			
	28	0.245	— —	18.67		0.000057	30	63.3	+ 1.3	+0.0403			
	29	1.21	— —	19.88		0.001228	30	59.2	— 2.8		−0.0869		
	30	— —	0.165	19.715		0.000028	30	66.2	+ 4.2	+0.1303			
	31	0.14	— —	19.855		0.000024	30	73.8	+11.8	+0.3660			
	32	— —	0.12	19.735		0.000019	30	77.6	+15.6	+0.4839			
	33	— —	0.14	19.595		0.000024	30	73 7	+11.7	+0 3629			
	34	1.21	— —	20.805		0.001228	30	68 8	+ 6.8	+0.2109			
	35	1.405	— —	22.21		0.001653	30	65.8	+ 3.8	+0.1179			
	36	2.34	— —	24.55		0.004571	30	65.5	+ 3.5	+0.1086			
	37	— —	1.085	23.465		0.000985	30	61.5	— 0 5		−0.0155		
	38	0.47	— —	23.935		0.000192	30	59.4	— 2.6		−0.0807		
	39	0.525	— —	24.46		0.000238	30	55 6	— 6.4		−0.1985		
	40	1.265	— —	25.725		0.001341	30	55.1	— 6.9		−0.2140		
	41	1.18	— —	26.905		0.001168	30	56.1	— 5.9		−0.1830		
	42	— —	0.19	26.715		0.000036	30	58.4	— 3.6		−0.1117		
	43	1.565	— —	28.28		0.002049	30	58.2	— 3.8		−0.1179		
	44	1.485	— —	29.765		0.001845	30	57.3	— 4 7		−0.1458		
	45	0.24	— —	30.005		0.000055	30	60.8	— 1.2		−0.0372		
	46	1.26	— —	31.265	12.130	0.001973	20	65.3	+ 3.3	+0.0682			
	400 ft.												
		40.44	9.175	31.265	31.265	0.071401	1369+ .925521			+5.9890	−1.8023	+4.1867	

Hypothenuſal length of the baſe containing 1369.925521 glaſs rods of twenty feet each + 4.31 feet,		27402.8204
Reduction contained in the ſeventh column to be ſubtracted,	−	0.0714
Total apparent length of the baſe reduced to the level of the ſouth-eaſt extremity,		27402.7490
Add to the apparent length the difference between the expanſion of glaſs above, and the contraction of it below 62°, contained in the thirteenth column = 4.1867 inches =	+	0.3489
Add further to the apparent length the equation for 6° difference of temperature of the ſtandard braſs ſcale between 62° and 68°, the heat in which the glaſs rods were laid off = 20.3352 inches, =	+	1.6946
Correct length of the baſe in the temperature of 62°, reduced to the level of the lowermoſt extremity,		27404.7925
Reduction for the height of the lower end of the baſe above the mean level of the ſea, ſuppoſed to be 54 feet or 9 fathoms =	−	0.0706
True length of the baſe reduced to the mean level of the ſea,		27404.7219

Table of the Expansions of Metals, deduced from experiment

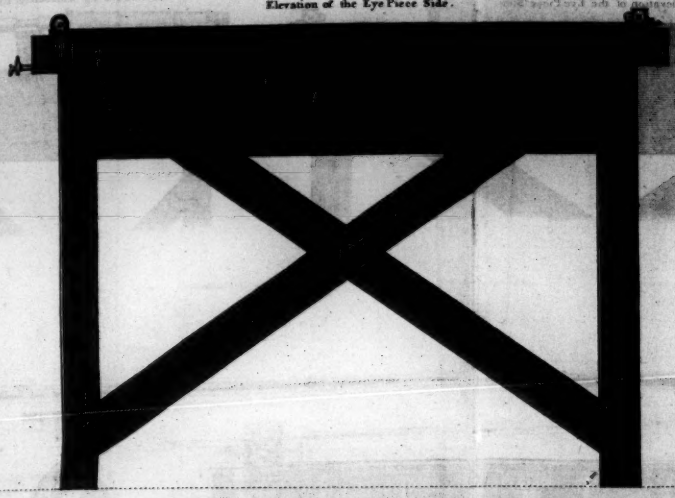
Description of the Metal Rods put to experiment.	Revolutions and parts of the micrometer for the expansion on five feet.		Actual expansion in parts of inch by 180°, the revolutio being divided by 32.06.		
	By 180°	By 1°.	On 5 feet.	On 1 foot.	On 100
	Rev.Pts.	Parts.	In.	In.	In.
Standard brass scale. { Supposed to be Hamburgh plate brass; length 42.187 inches, or 3.568 feet; breadth 0.55 inch; thickness 0.25 inch; and weight 1 lb. 10½ oz. Its expansion was measured by 25.47 revolutions of the micrometer; wherefore that on five feet would have been measured by - }	35.69	19.$\frac{33}{100}$	0.111323	0.0222646	2.2264
English plate brass, in form of a rod. { Length five feet; breadth 0.9 inch; thickness 0.15 inch; and weight 2 lbs. 5½ oz. Difficult from its thinness to be kept free from warping, - - - }	36.41	20.$\frac{33}{100}$	0.113568	0.0227136	2.2713
English plate brass, in form of a trough. { Length five feet; breadth 1.4 inch; depth 1 inch; weight 8 lbs. 3 oz. Perfectly strong and straight, - - - }	36.45	20.$\frac{33}{100}$	0.113693	0.0227386	2.2738
Steel rod. { Length five feet; breadth 0.5 inch; thickness 0.3 inch; weight 2 lbs. 7½ oz. Made from the very same bar with the chain, - - }	22.02	12.$\frac{32}{100}$	0.068684	0.0137368	1.3736
Cast iron prism. { Length five feet; each of its sides 1¼ inch; and weight 11 lbs. 9 oz Cut from the same rod with the standard prisms of the pyrometer, }	21.34	11.$\frac{66}{100}$	0.066563	0.0133126	1.3312
Glass tube. { Length five feet; $\frac{30}{100}$ths inch diameter; weight 1 lb. 13½ oz. Drawn from the same pot of metal with the measuring rods, - }	14.93	8.$\frac{39}{100}$	0.046569	0.0093138	0.9313
Solid glass rod. { Length 40.44 inches, or 3.37 feet; mean diameter six-tenths of an inch; and weight 1 lb. 2 oz. It had been applied for several years to a clock. Its expansion was measured by 10.46 revolutions of the micrometer; wherefore that on five feet would have been measured by - }	15.54	8.$\frac{63}{100}$	0.048472	0.0096944	0.9694

iments made with the Microscopic Pyrometer in April 1785.

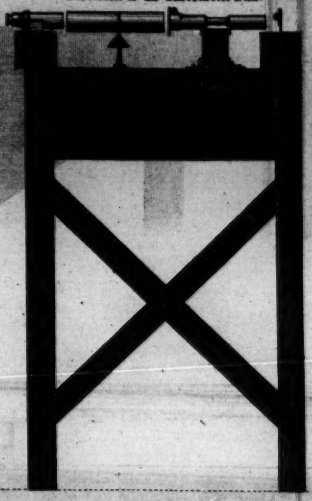
rts of an volutions	By 1° of FAHRENHEIT the expansion is						Bases of 27400 feet of these metals would expand.			
On 100 ft.	On 1 foot.	10 feet.	100 feet.	400 feet.	600 feet.	1000 ft.	By 1°.	By 10°.	By 20°.	By 30°.
n.	In.	In.	In.	In.	In.	In.	In.	In.	In.	In.
2.22646	0.0001237	0.001237	0.01237	0.04948	0.07422	0.1237	3.38938	33.8938	67.7876	101.6814
2.27136	0.0001262	0.001262	0.01262	0.05048	0.07572	0.1262	3.45788	34.5788	69.1576	103.7364
2.27386	0.0001263	0.001263	0.01263	0.05052	0.07578	0.1263	3.46062	34.6062	69.2124	103.8168
1.37368	0.0000763	0.000763	0.00763	0.03052	0.04578	0.0763	2.09062	20.9062	41.8124	62.7186
1.33126	0.0000740	0.000740	0.00740	0.02960	0.04440	0.0740	2.02760	20.2760	40.5520	60.8280
0.93138	0.0000517	0.000517	0.00517	0.02068	0.03102	0.0517	1.41658	14.1658	28.3316	42.4974
0.96944	0.0000539	0.000539	0.00539	0.02156	0.03234	0.0539	1.47686	14.7686	29.5372	44.3058

For the DESCRIPTION and USE of th

Elevation of the Eye Piece Side.

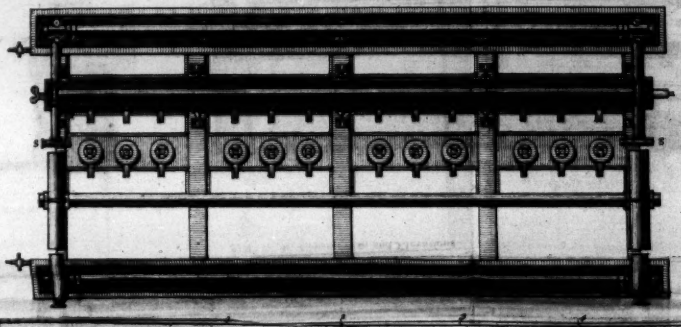


Elevation of the Micrometer End.

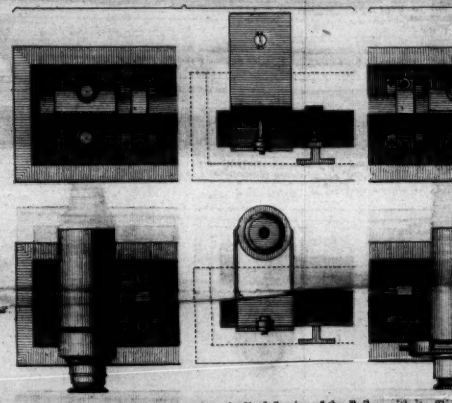


PLANS and ELEVATIONS of the MARKS and EYE-PI

General Plan of the Top.



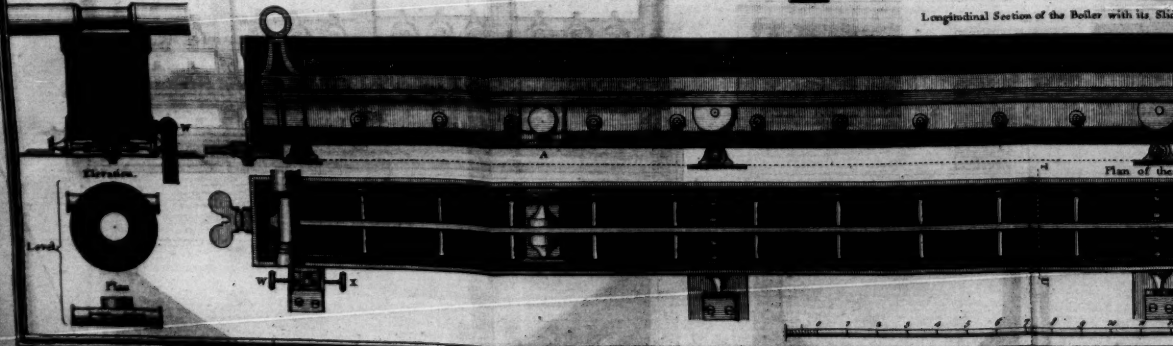
Fixed Microscope.



Scale to the General Plan and Elevations.

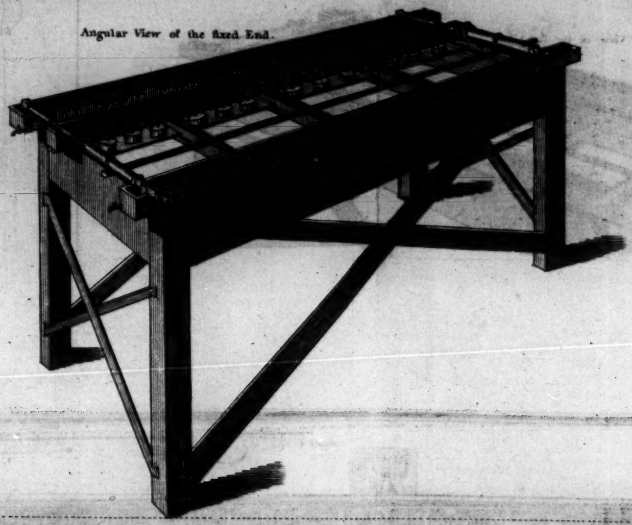
Elevation of the fixed end of the Boiler.

Longitudinal Section of the Boiler with its Sli



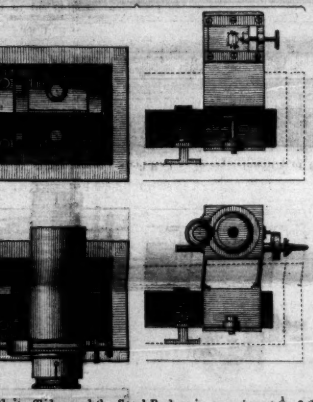
of the MICROSCOPIC PYROMETER.

Angular View of the fixed End.

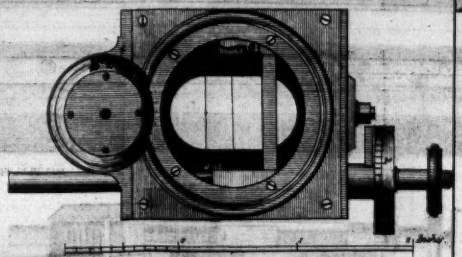


EYE-PIECES $\frac{1}{4}$ of the real size.

Micrometer Microscope.

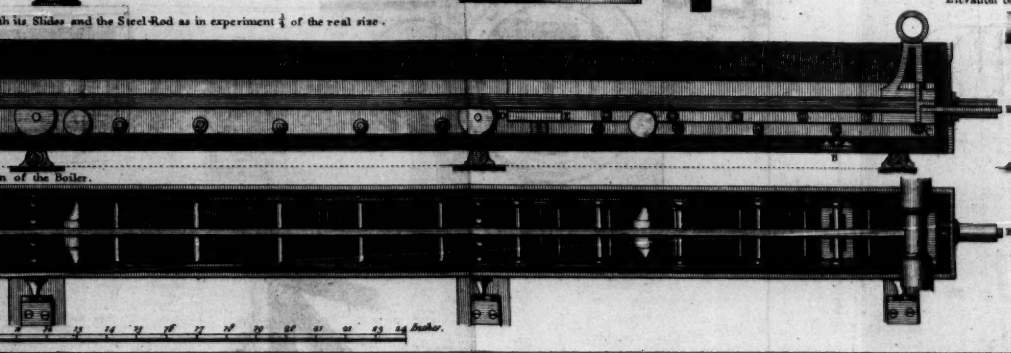


Elevation of the Micrometer full size.



Horizontal Section of the Micrometer.

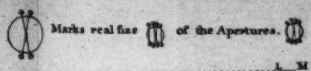
th its Slides and the Steel-Rod as in experiment $\frac{1}{4}$ of the real size.

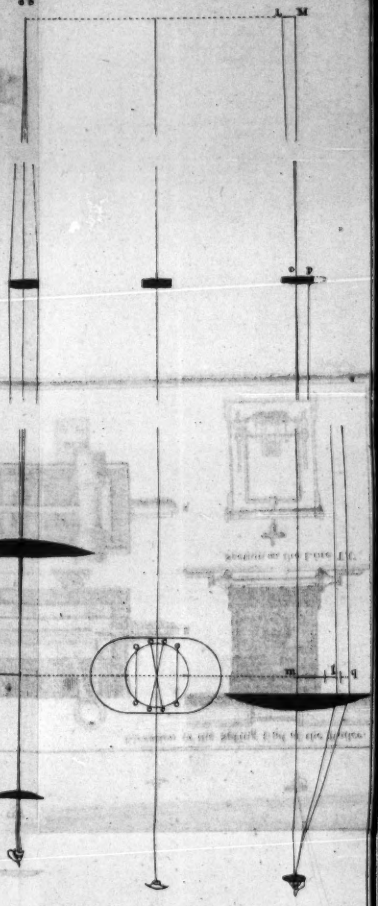


n of the Boiler.

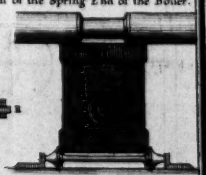
CONSTRUCTION of the MICROSCOPES.

Marks real size of the Apertures.

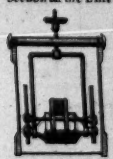




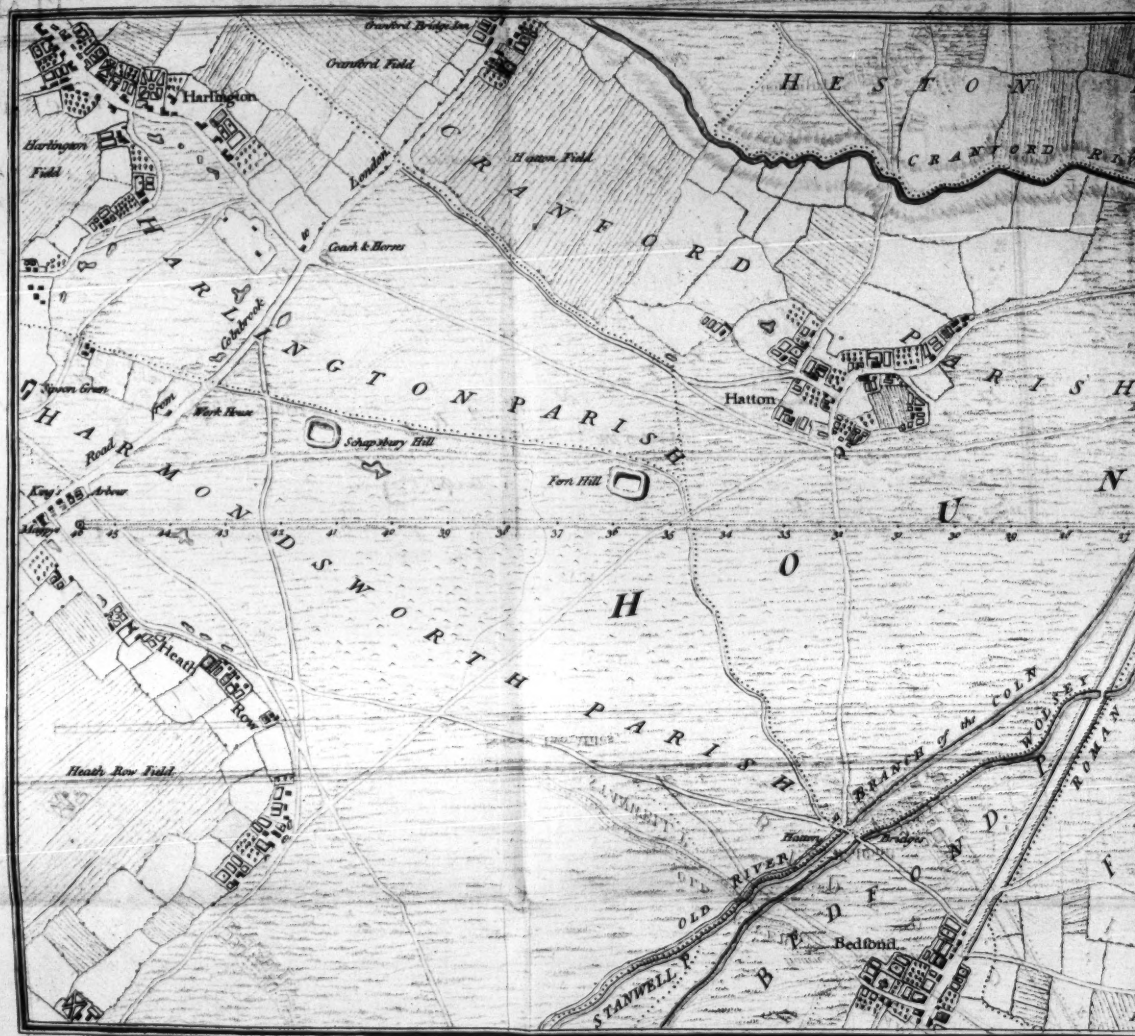
Elevation of the Spring End of the Boiler.



Section on the Line T.U.

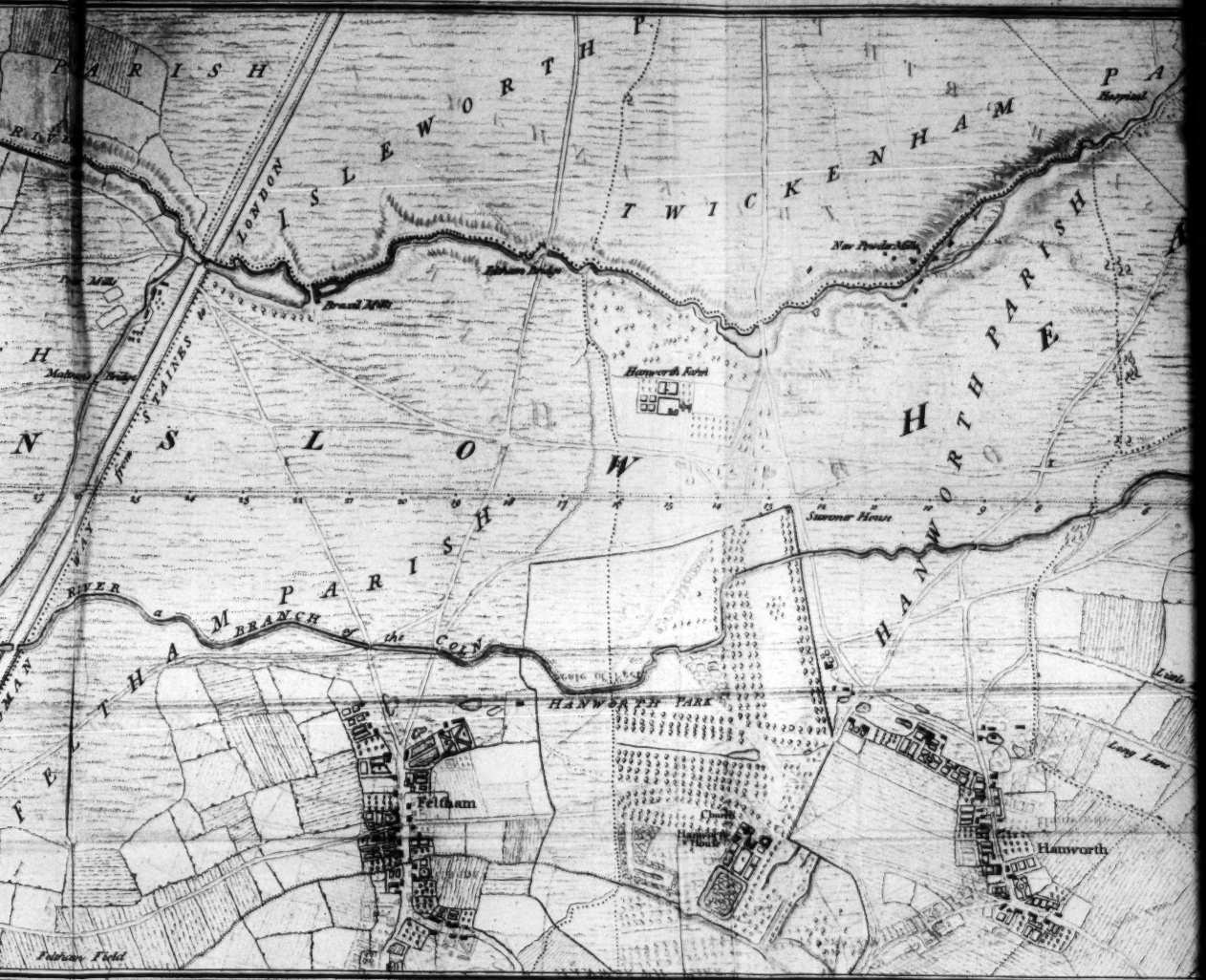


PLAN shewin

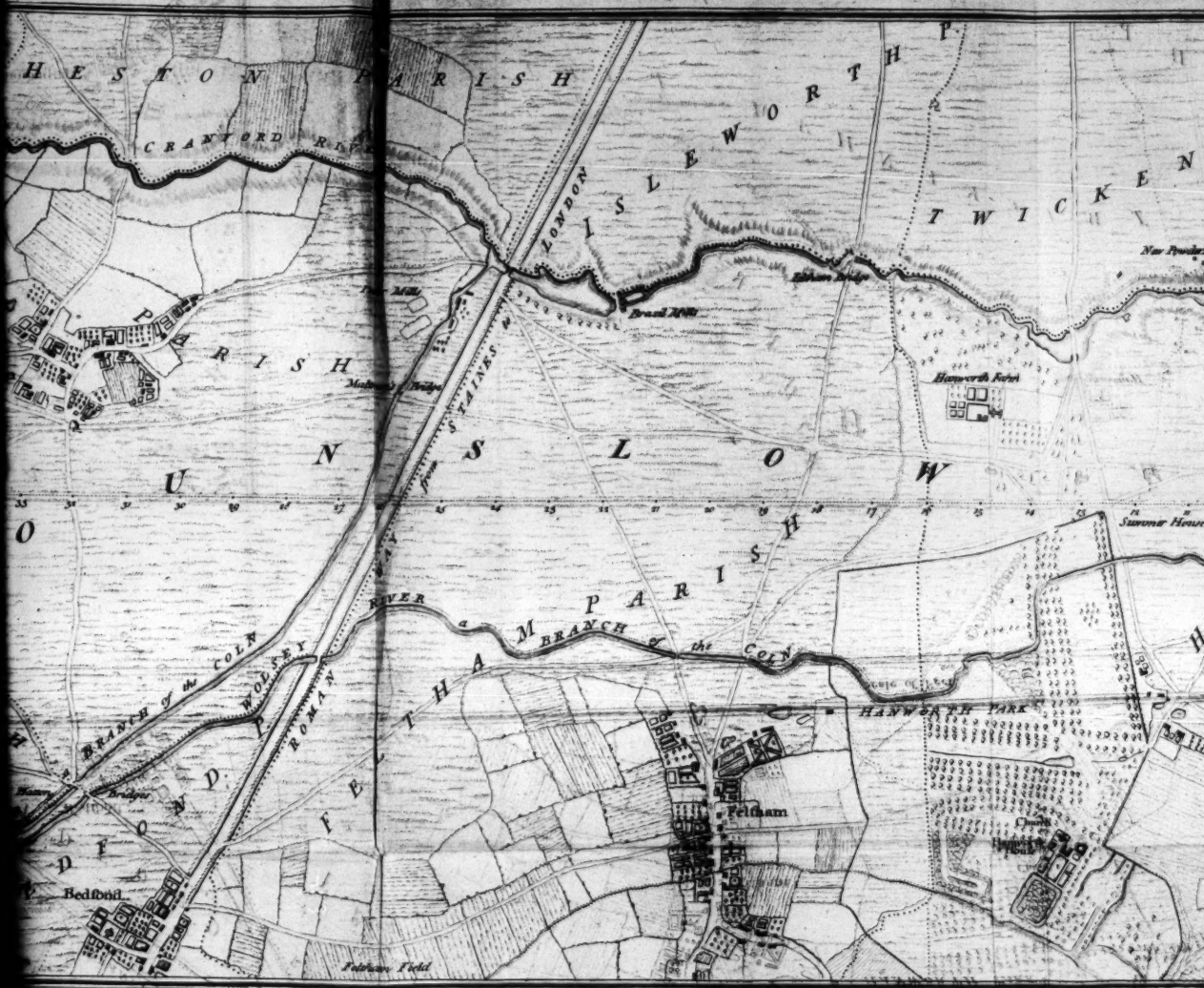


Scale of Two Miles.

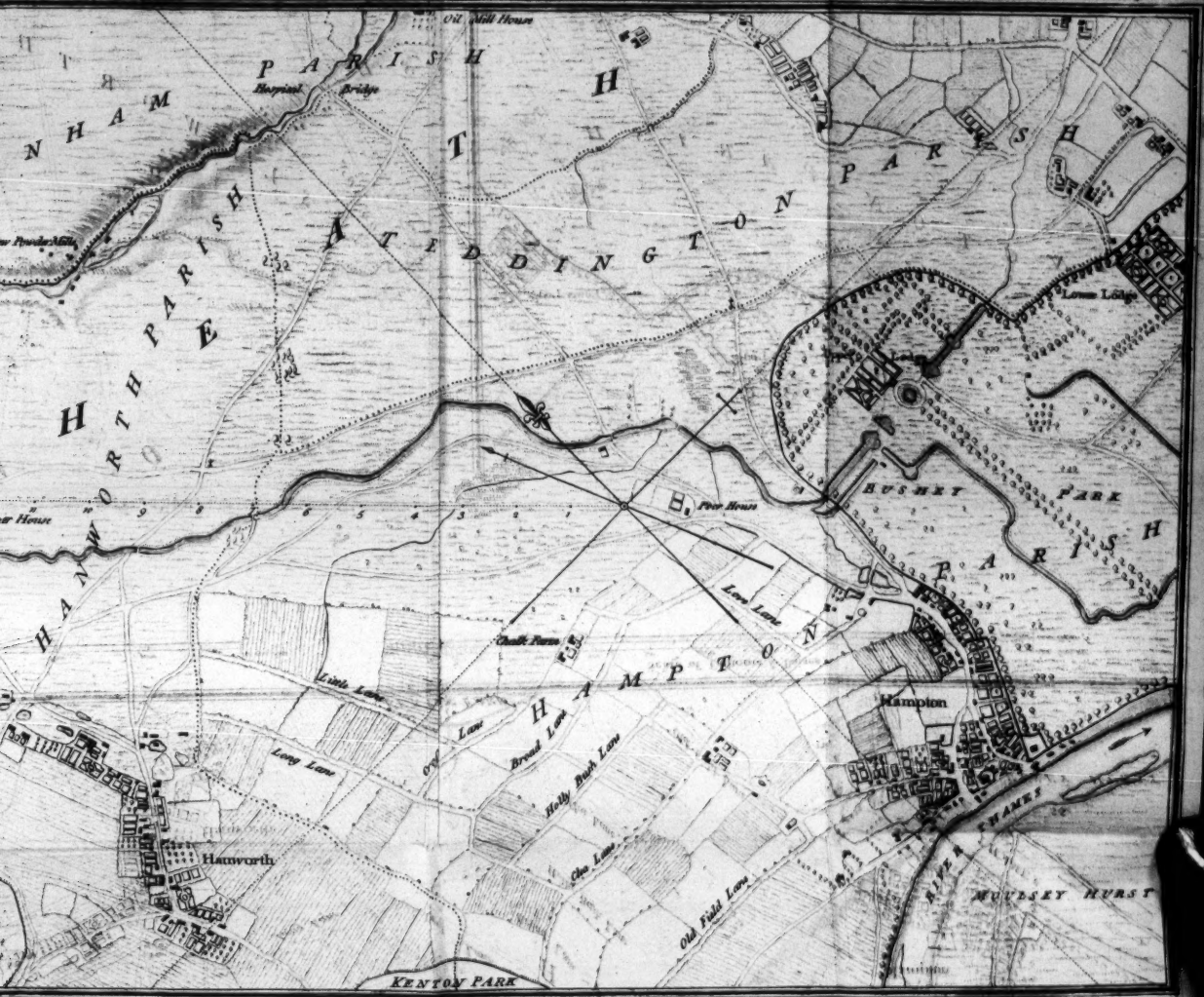
hewing the situation of the BASE measured on HOUNSLOW HEATH in Summer 1784.

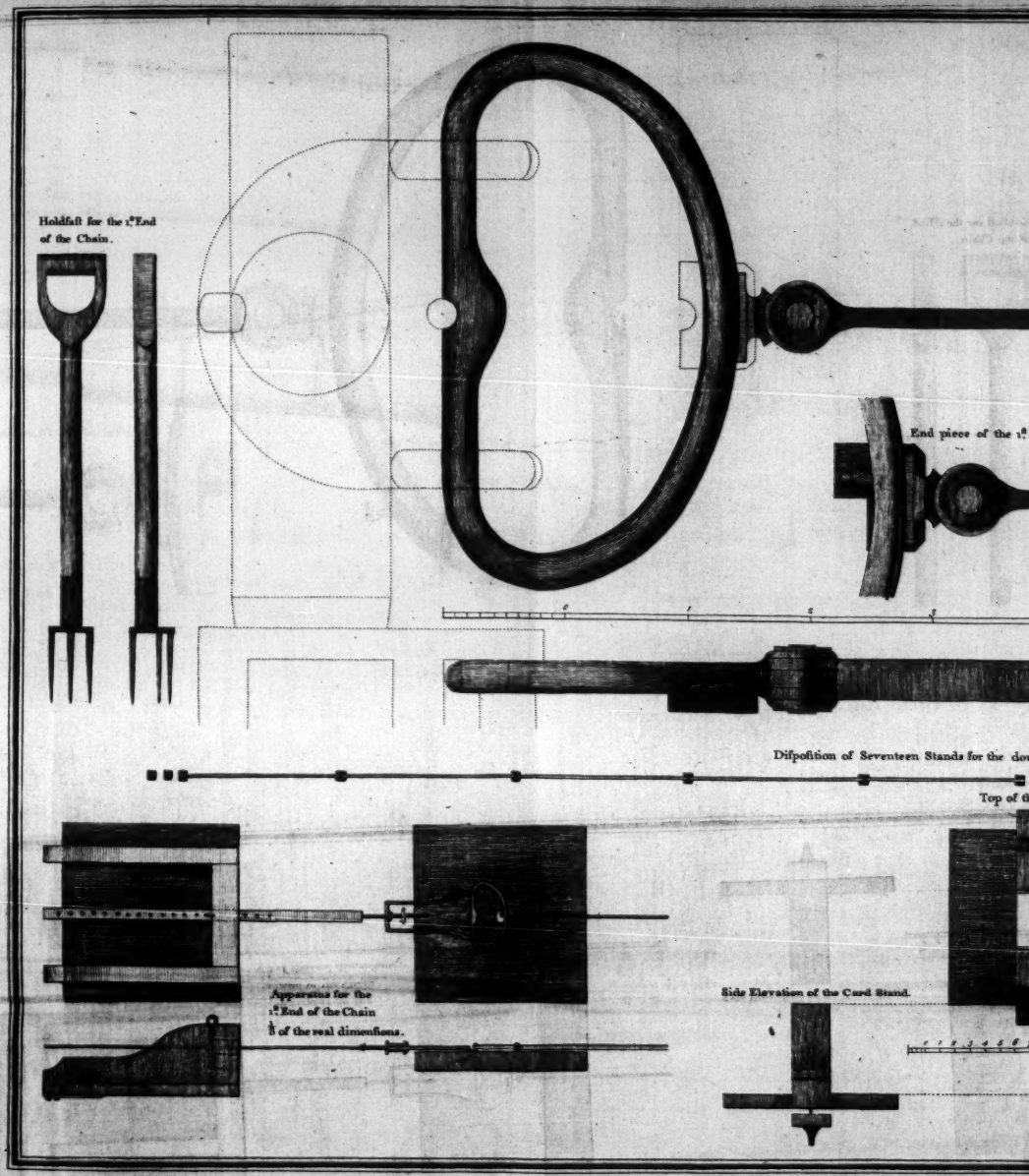


PLAN ſhewing the ſituation of the BASE meaſured on HOUNSLOW HEATH



f Hum Summer 1784.




Holdfast for the 1st End of the Chain.
End piece of the 1st
Disposition of Seventeen Stands for the doo
Top of t
Apparatus for the 1st End of the Chain 1/8 of the real dimensions.
Side Elevation of the Card Stand.

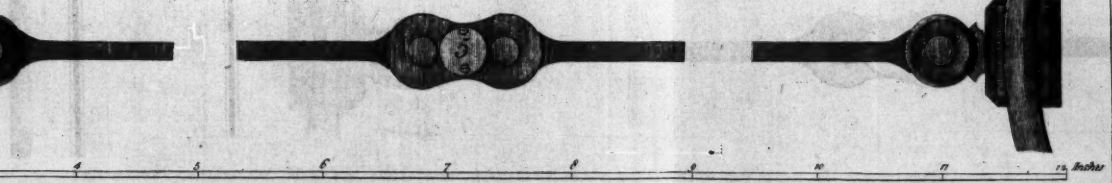
For the DESCRIPTION and APPLICATION of the STEEL CHAIN.

Plan of the 1st Link of the Chain in its original State.

of the 1st Link in its altered State.

Plan of the Cross joint at every 10th Link.

End piece of the last Link in its altered State.



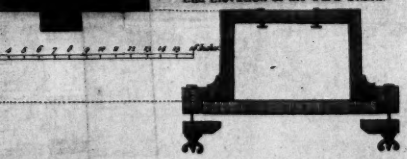
4 5 6 7 8 9 10 11 12 Inches

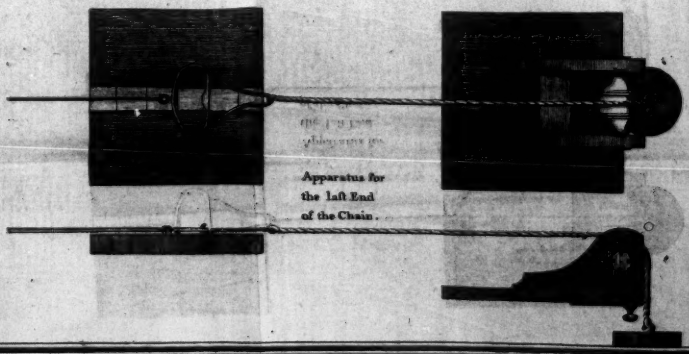
Elevation of the 1st Link in its original State.

r the double Measurement with the Chain and Glass Rods.

Top of the Card Stand.

End Elevation of the Card Stand.



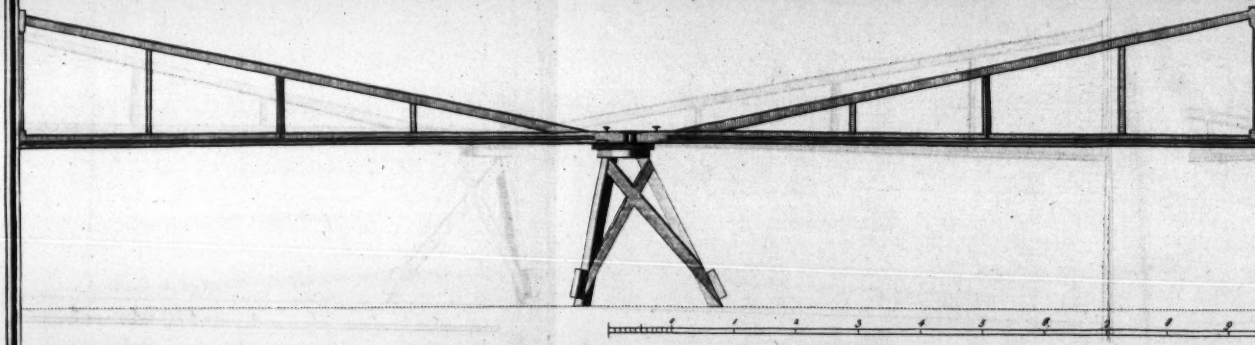


Apparatus for the last End of the Chain.

Basire sc.

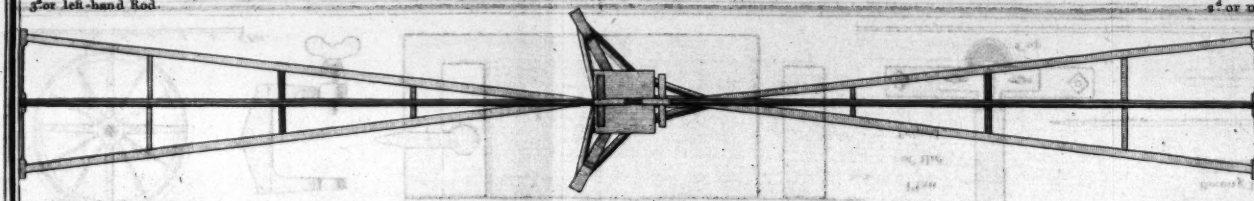
For the DESCRIPTION and APPL

Subdivision of the Ground of one Hypothenuse of 600 Feet into 30 Rod L

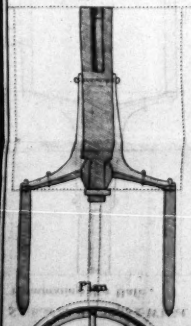


3d or left-hand Rod.

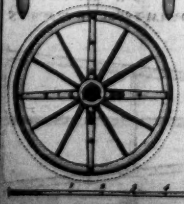
2d or r



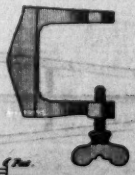
Section of the Pipe & Wheel terminating the Base.



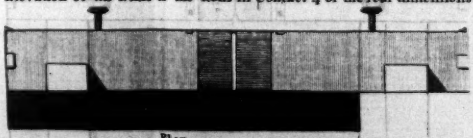
Plan



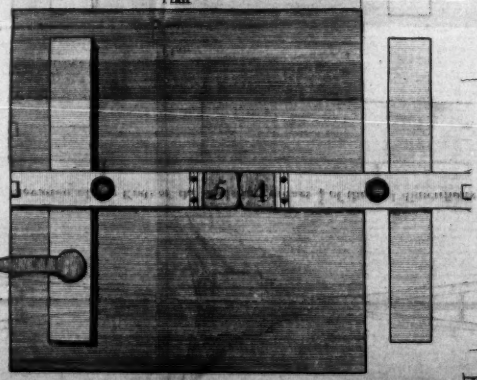
Vertical Clamp.



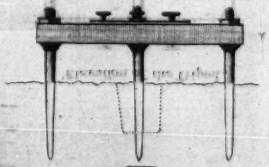
Elevation of the Ends of the Rods in Contact ¼ of the real dimensions.



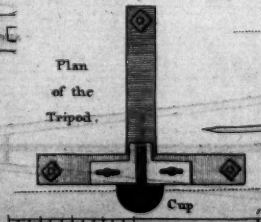
Plan.



Elevation of the Tripod.



Plan of the Tripod.

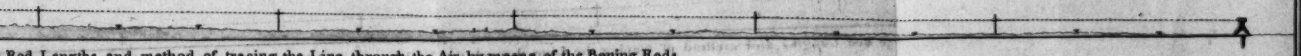


Cup

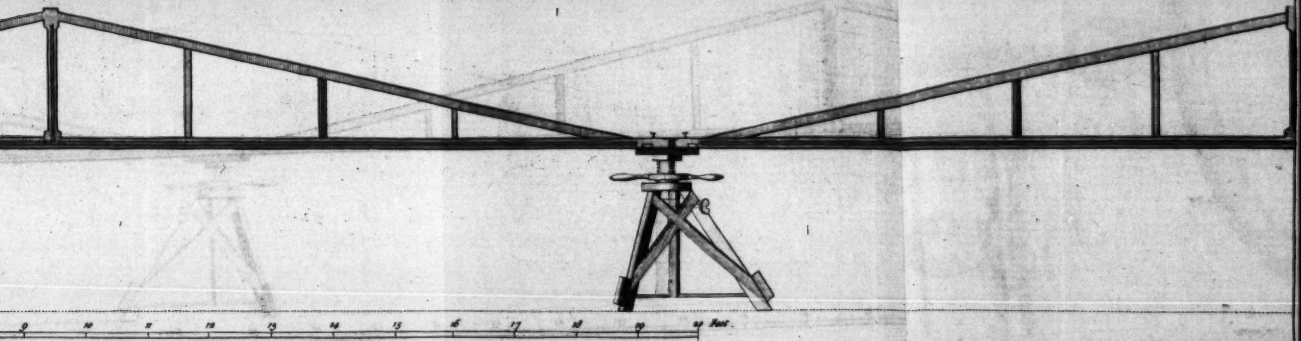
Elevation th

Boring

PPLICATION of the DEAL RODS &c.

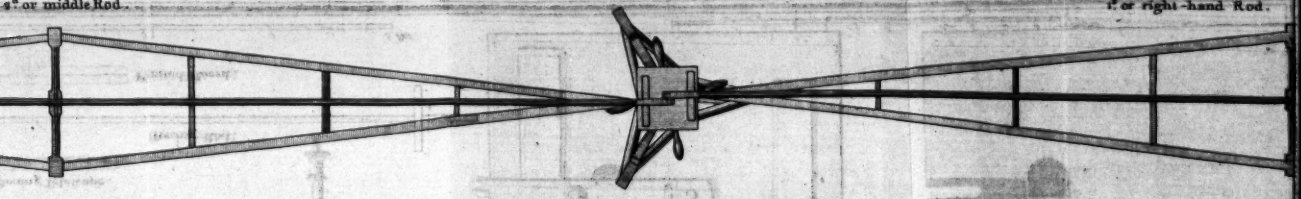


Rod Lengths, and method of tracing the Line through the Air by means of the Boning Rods.

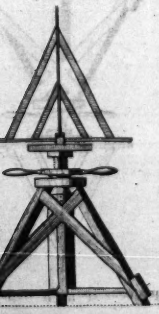


4d or middle Rod.

1st or right-hand Rod.



tion the Rods seen endwise.



Horizontal Clamp.

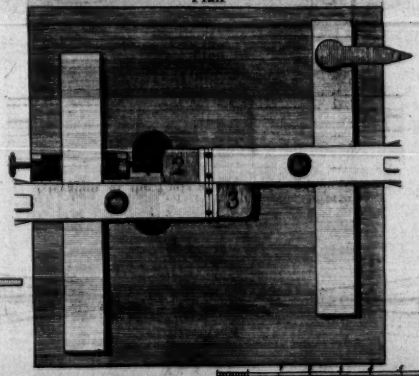
Boning Telescope.

Boning Rod.

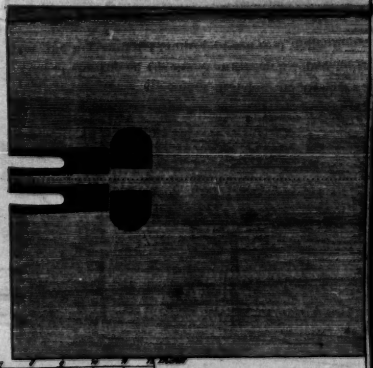
Painted Board.

Elevation of the Ends of the Rods in Coincidence.

Plan



Plan of the Top of the Stand shewing the Grooves for the horizontal Clamp $\frac{1}{4}$ of the real dimensions.



For the DESCRIPTION and APPLICA

1st or left-hand Rod.

2d or middle

Scale of

Longitudinal Elevation ¼ of the real dimensions.

1st Rod

2d Rod

Front Elevation of the moveable End.

1st Rod

Longitudinal Section of the moveable End of the 1st Rod and fixed End of the 2d Rod in their real dimensions.

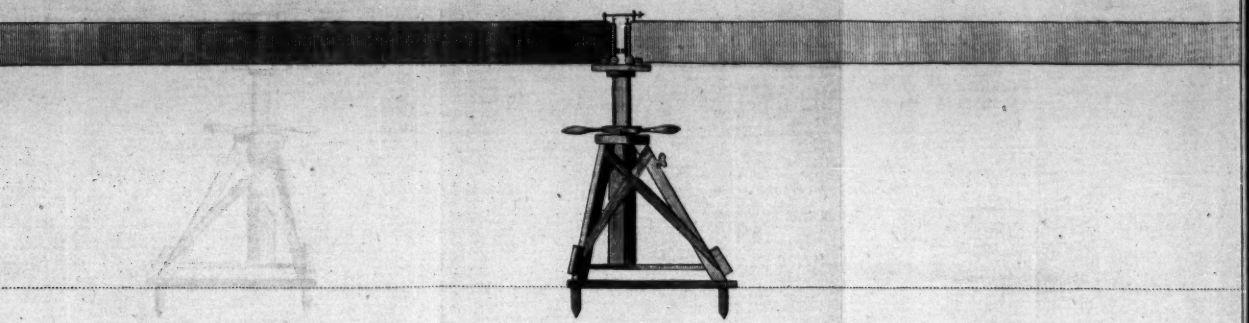
Section across the middle of ¼ of the real dimensions.

Plan

Plan

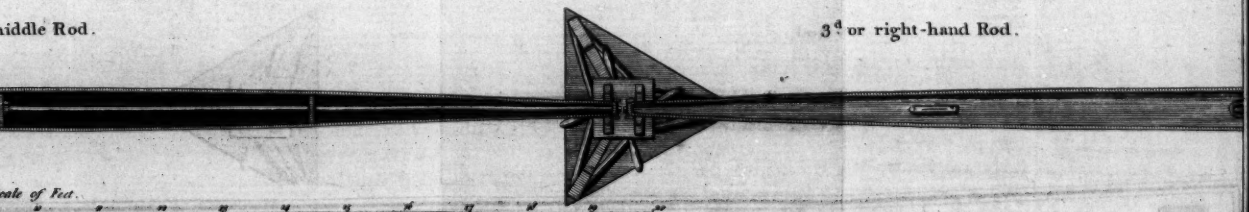
Scale of Inches for parts represented in their full

PLICATION of the GLASS RODS.



iddle Rod.

3d or right-hand Rod.



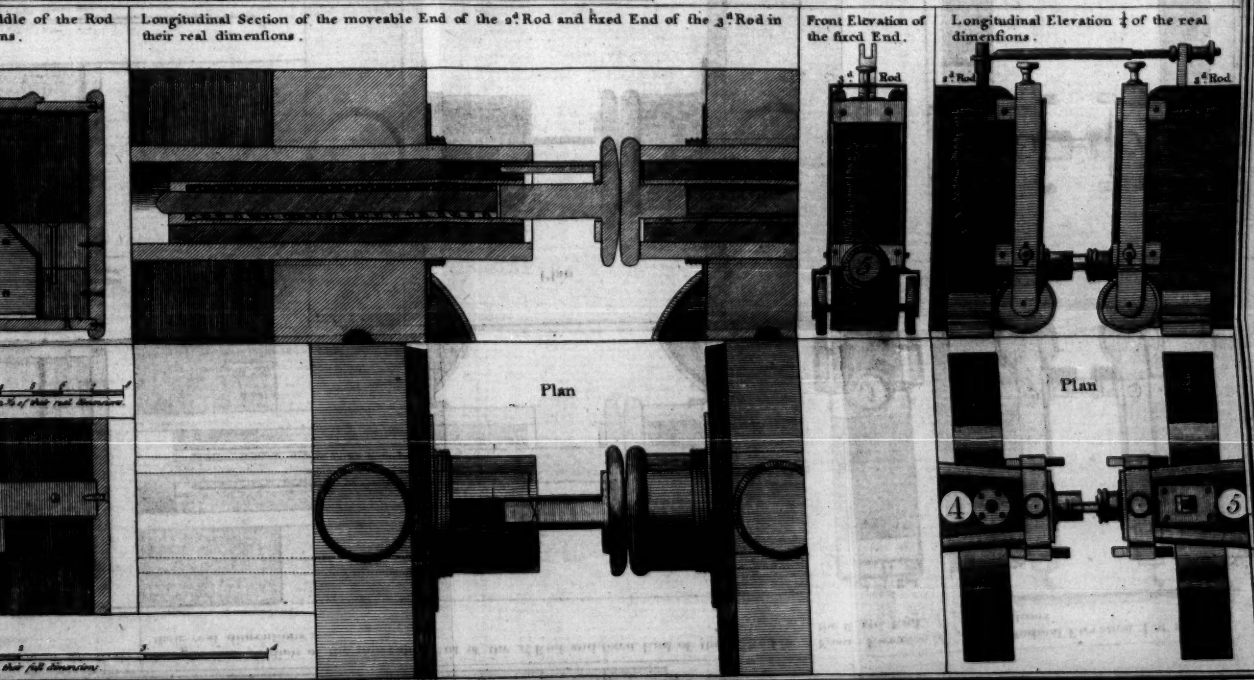
cale of Feet.

dle of the Rod ns.

Longitudinal Section of the moveable End of the 2d Rod and fixed End of the 3d Rod in their real dimensions.

Front Elevation of the fixed End.

Longitudinal Elevation ⅛ of the real dimensions.



their full dimensions.

XXV. *Abſtract of a Regiſter of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1784. *By* Thomas Barker, *Eſq. Alſo of the Rain at* South Lambeth, Surrey; *and at* Selbourn *and* Fyfield, Hampſhire. *Communicated by* Thomas White, *Eſq. F.R.S.*

Read June 16, 1785.

		Barometer.			Thermometer.						Rain.			
					In the Houſe.			Abroad.						
		Higheſt	Loweſt	Mean.	High.	Low.	Mean	High.	Low.	Mean	Lyndon	S. Lambeth, Surrey.	Selbourn, Hants.	Fyfield.
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,96	28,49	29,34	40½	28	33½	40	15½	27	1,877	2,54	3,18	2,44
	Aftern.				42½	29	34	48½	24½	32½				
Feb.	Morn	30,00	28,50	29,23	47	30	36	45	9	29	1,225	1,49	0,77	1, 7
	Aftern				47½	31	37	52½	23	36				
Mar.	Morn.	29,63	28,59	29,23	47½	36	39½	45	21	32½	1,096	2,63	3,82	2,24
	Aftern.				48½	36	40½	52	33¼	40½				
Apr.	Morn.	29,74	28,44	29,26	50½	35½	43½	51	29	38½	1,741	2,56	3,92	2,10
	Aftern.				53	36	45	60½	35	48				
May	Morn.	29,92	29,17	29,62	66½	47	57½	63	41	52	2,890	1,36	1,52	1,57
	Aftern.				69½	48	59½	78	48½	65				
June	Morn.	29,92	28,98	29,43	62	55½	58	61½	48	54	3,810	3,45	3,65	2,45
	Aftern.				63½	56½	59½	71	53	63½				
July	Morn.	29,85	28,74	29,48	69	56	61	66	51	56	5,080	2,26	2,40	2,80
	Aftern.				72	57	63	79½	57½	67				
Aug.	Morn.	29,92	29,04	29,56	65	54	59	60½	42½	52	2,814	2,84	3,88	2,79
	Aftern.				67	55	60	71½	51	63				
Sept.	Morn.	29,90	29,01	29,55	66½	53	61	57	39	52	1,740	1,65	2,51	2, 7
	Aftern.				71½	54	63	73½	51½	64				
Oct.	Morn.	30,00	28,98	29,62	52½	42	48½	45	27½	39½	0,223	0,83	0,39	0,17
	Aftern.				53½	43	49½	55½	40	49				
Nov.	Morn.	29,85	28,75	29,38	51	39½	45	51½	23½	38	2,376	5,60	4,70	3,14
	Aftern.				51	41	45½	53	33½	44				
Dec.	Morn.	29,75	28,15	29,26	43½	31½	36½	41	13½	29	2,335		3,06	1,72
	Aftern.				43	32½	37½	46½	19	32½				
											Inches 27.207	27,21	33,80	24,56

The froſt, which began at Chriſtmas laſt year, continued, except a few thawing days, till February 21, and was very ſevere, and with frequent ſnow. Some thought it the hardeſt winter ſince 1740; but that may be hard to determine. January 1776 ſeems to have been rather colder than this; but the froſt laſted only a month. In 1780 the froſt was not out of the ground for nine or ten weeks; but it was not ſo ſteady as this. It was certainly one of the mildeſt winters before Chriſtmas, and one of the ſevereſt after it; yet the corn and other plants did not ſuffer ſo much as might be feared. After the froſt there was ſome warm, windy, ſhowery weather; but moſt part of March was froſty mornings, and was often ſo in the ſhade all day; and at the latter end ſtrong cold winds, with ſnow and perfect winter; and it did not much mend till near the middle of April.

While in January and February moſt parts of Europe had ſevere froſt, the ſouthern parts of it ſeem to have had great ſtorms and floods; and, at the breaking of the froſt, the flat countries by the ſides of the great rivers of Europe ſuffered much by floods and ice.

The latter half of April the weather mended, and things came on gradually, yet with frequent froſty mornings till the firſt week in May; then, for three weeks, one of the fineſt and hotteſt Mays ever known; every thing before was exceeding backward, but now came on at a vaſt rate; the graſs and leaves were remarkably green, a great bloſſom year, and plenty of fruit. This hot weather brought up thunder, which turned the weather wet near the end of May, and it was wet or ſhowery and cool all June; this brought on the corn again, which was made rather thin by ſo much heat too early. Near the firſt two thirds of July was again fine and hot, and being

in

in the height of hay time, a good deal of it was well made; but ſome of the firſt cut, and the latter was caught in the wet; for after the 19th it was ſhowery or wet, and the 30th and 31ſt were, all over Leiceſterſhire, Rutland, and part of Lincolnſhire, the greateſt flood ſince July 1736; and it continued wet and cool all Auguſt, ſo that the ſummer was in general cold, wet, and backward, yet with ſome very fine fits in it. The harveſt began but indifferently; but being late this year, and the weather wet, not much was carried before September, when, in about three weeks calm, hot, and dry weather, yet with vaſt dews, moſt of the white corn was well got in this country; but ſome of the peaſe, and, where it was earlier and later, ſome of the white corn was carried damp, for the end of September was again wet.

The autumn was various; a dry and fine October, toward the end of it ſharp froſty mornings; a ſhowery November, with a ſharp froſt in the middle, yet often pleaſant; and after December 5th, a conſiderable ſnow (in ſome countries it was very great) and a ſeverer froſt than is uſual before Chriſtmas laſted till into January.

PRESENTS

MADE TO THE

ROYAL SOCIETY

From Auguſt 1784 to June 1785;

WITH

The NAMES of the DONORS.

	Preſents.	Donors.
1784.		
Aug. 12.	Oeuvres de Charles Bonnet. Tom. VI. VII. VIII. Neufchatel. 1782, 1783. 4°	The Author.
——	Eſſay II. on the Nature and Principles of Public Credit. St. Auguſtine, Eaſt Florida. 1784. fol.	S. Gale, Eſq.
Nov. 4.	Deſcription géométrique de la France. Par M. Caſſini de Thury. Paris. 1783. 4°	The Author.
——	Rapport des Commiſſaires chargés par le Roi de l' examen du Magnétiſme Animal. Paris. 1784. 4°	The Authors.
——	Tableau méthodique des Minéraux. Par M. Daubenton. Paris. 1784. 8°	The Author.
——	Lettre ſur les Expériences des Frictions glaciales pour la guériſon de la Peſte. Par M. D. Samoïlowitz. Paris. 1781. 8°	The Author.
——	Lettre à l'Académie de Dijon avec réponſe à ce qui a paru douteux dans le Mémoire ſur l'Inoculation de la Peſte. Par M. D. Samoïlowitz. Paris. 1783. 8°	The Author.

1784.	Presents.	Donors.
Nov. 4.	Réponse a M. Gormand, sur la question: "*Si les Cautères, peuvent être quelque préservatif contre la Peste pendant ses ravages.*" Par M. D. Samoilowitz. 12°	The Author.
——	Die bestimmung der Gestalt und Grösse der Erde: von F. W. Gerlach. Wien. 1782. 8°	The Author.
——	The original Astronomical Observations made in the course of a Voyage to the Northern Pacific Ocean, by Capt. James Cook, Lieut. James King, and Mr. William Bayly. London. 1782. 4°	The Commissioners of Longitude.
——	Annals of Agriculture. By Arthur Young, Esq. London. 1784. 8°	The Author.
——	A Treatise on the Diseases of Children. By Michael Underwood, M. D. London. 1784. 12°	The Author.
Nov. 11.	Dell' Architettura Dialogi: di Ermenegildo Pini. Milano. 1770. 4°	The Author.
——	Offervazioni mineralogiche su la Miniera di Ferro dell' Isola d'Elba: di E. Pini. Milano. 1777. 8°	The Author.
——	Mémoire sur des nouvelles Cristallisations de Feldspath. Par E. Pini. Milan. 1779. 8°	The Author.
——	Hermenegildi Pini de Venarum metallicarum excoctione Vol. I. et II. Mediolani. 1779, 1780. 4°	The Author.
——	Memoria mineralogica sulla Montagna e sui contorni di S. Gottardo: di E. Pini. Milano. 1783. 8°	The Author.
——	Descrizione di un Pantaulo: di E. Pini. Milano. 1783. 8°	The Author.
——	Della maniera di osservare nei Monti la disposizione degli Strati: di E. Pini. 4° Dell' Elevazione dei principali Monti della Lombardia Austriaca: di E. Pini. 4° Osservazioni mineralogiche sulla Montagna di S. Gottardo: di E. Pini. 4°	The Author.
——	Mémoires et Observations de Chimie. Par M. de Fourcroy. Paris. 1784. 8°	The Author.
——	Observations sur la Cure de la Gonorrhée. Trad. de l'Anglois de M. S.F. Simmons;	The Translator.

		Presents.	Donors.
1784.			
	par Gabriel Mauvet. Montpellier. 1783.	8°	
Nov. 11.	Medical Communications. Vol. I. London. 1784.	8°	The Society for promoting Medical Knowledge.
	18. Obſervations on the late Conteſts in the Royal Society. By Andrew Kippis, D. D. London. 1784.	8°	The Author.
Dec.	9. An Eſſay on the Uſe of the Red Peruvian Bark. By Edward Rigby. London. 1783.	8°	The Author.
——	An Eſſay on the uterine Hæmorrhage which precedes the Delivery of the full-grown Fœtus. By E. Rigby. Lond. 1784.	8°	——
——	An Aſtronomical MS. in the Perſic Language.	8°	Major Pierſon.
	16. Deſcription des Projets et de la Conſtruction des Ponts, &c. Par M. Perronet. Tom. I. II. Paris. 1782, 1783.	fol.	The Author. ——
——	Deſcription of a Method of taking the Differences of Right Aſcenſion and Declination with the Reticule Rhomboide of Dr. Bradley. By H. E. Bath.	4°	Sir Henry C. Englefield, Bart. ——
——	An Enquiry into the various Theories and Methods of Cure in Apoplexies and Palſies. By B. Chandler, M. D. Canterbury. 1785.	8°	The Author.
	23. A meteorological Journal kept at Fort William, Calcutta. MS.	fol.	Major Pearce. ——
1785.			
Jan.	13. A Treatiſe on Time. By W. Watſon, jun. M. D. London. 1785.	8°	The Author.
	20. The London Medical Journal. Vol. V. London. 1785.	8°	S. F. Simmons, M. D.
——	An Eſſay on Comets. By Andrew Oliver, jun. Salem, New-England. 1772.	8°	The Rev. Profeſſor Williams.
Feb.	3. Meteorological Journals kept at Nain, Okkak, and Hoffenthal, in Labrador. MS.	4°	The Rev. Mr. La Trobe. ——
	10. An Account of the Muſical Performances in Commemoration of Handel. By C. Burney, Muſ. D. London. 1785.	4°	The Author and the Muſical Society. ——
	17. Obſervations ſur la Phyſique, ſur l'Hiſtoire naturelle, et ſur les Arts. Par M. l'Abbé		The Editors.

	Presents.	Donors.
1785.		
	Rozier, et par M. J. A. Mongez le jeune. Tom. XXIV. Part. 1784. 4°	
Feb. 24.	An Inquiry how to prevent the Small-Pox, &c. By John Haygarth, M. B. Chester. 1784. 8°	The Author.
Mar. 10.	Remarks on morbid Retentions of Urine. By C. Brandon Trye, Glocester. 1774. 8°	The Author.
17.	A Portrait of the Elector Palatine Duke of Bavaria.	His most serene Highness the Elector Palatine.
——	Quatrième Lettre du Chev. Rosa sur ses Découvertes Physiologiques (translated from the Italian) MS. fol.	The Marquis de Hautefort.
——	Commentarii de Rebus in Scientia naturali et Medicina gestis. Vol. XXV. Lipsiæ. 1782. 8°	Mr. Philip Hurlock.
——	Connoissance des Temps, pour l'Année 1787. Par M. Jeaurat. Paris. 1784. 12°	The Author.
——	Mémoire sur une Machine propre à creuser et curer les Canaux, Rivières ou Ports, Par les Freres F. et A. Eckhardt. 4°	M. A. Eckhardt.
——	Succinct Account of the Advantages of Mills with inclined float board Wheels. By Mr. A. G. Eckhardt. fol.	The Author.
April 7.	The Theory and Practice of Aerostation. By Tiberius Cavallo. London. 1785. 8°	The Author.
——	Observations on the Use of Opium in removing symptoms supposed to be owing to morbid irritability. By Alexander Grant. London. 1785.	The Author.
——	Opuscules de Pierre Richer de Belleval. Nouvelle Edition; par M. Broussonet. Paris. 1785. 8°	The Editor.
——	Lettre sur l'Anamephitique. Par M. Janin de Combe-Blanche. Vienne. 1783. 8°	The Author.
——	Seconde, troisième, et quatrième Lettres à M. Cadet. Par M. Janin de Combe-Blanche. Vienne. 1783, 1784. 8°	The Author.
——	L'Homme noyé dans une Fosse, a-t-il peri par le Méphitisme? Lettre par M. Janin de Combe-Blanche. 8°	The Author.

1785.	Presents	Donors.
April 7.	Preuves que l'Homme s'eſt noyé dans la foſſe; et que le Méphitiſme n'a pas cauſé ſa mort. Lettre par M. Janin de Combe-Blanche. 8°	The Author.
——	Nouvelles Expériences qui confirment celles qui ont été annoncées dans l'Antiméphitique. Par M. Janin de Combe-Blanche. 8°	The Author.
——	Réponſe au Diſcours de M. O-Rian ſur le Magnétiſme Animal. Par M. Janin de Combe-Blanche. Genève. 1784. 8°	The Author.
14.	Relief from accidental Death. By Alexander Johnſon, M. D. London. 1785. 12°	The Author.
28.	Three Regiſters of a Pocket Chronometer. By Count De Bruhl. 4°	The Author.
——	A Memoir on ſome Improvements in Bookbinding. By M. Aniſſon le fils (in French) MS. fol.	The Author.
May 5.	An Eſſay on the Nature and Cure of the Phthiſis pulmonalis, ſecond edit. By Thomas Reid, M. D. London. 1785. 8°	The Author.
——	Reports of the Humane Society for the years 1783 and 1784. 8°	The Humane Society.
——	Account of a Machine conſtructed for the purpoſe of deepening and ſcouring Canals, Rivers, and Ports. By the Brothers F. and A. Eckhardt. fol.	Mr. A. Eckhardt.
12.	Lettres I. II. III. IV. V. VI. ſur l'Architecture. Par M. Viel de Saint-Maux. Bruxelles. 1779. 1780. Paris. 1784. 8°	The Author.
26.	Diſſertatio Botanica de Sida. Auct. A. J. Cavanilles. Pariſiis. 1785. 4°	The Author.
——	Obſervations de M. l'Abbé Cavanilles ſur l'Article Eſpagne de la Nouvelle Encyclopédie. Paris. 1784. 8°	The Author.
——	Parallèle entre le Magnétiſme Animal, l'Electricité, et les Bains medicinaux. Par M. L***. Paris. 1785. 8°	M. Laugier.
——	Mémoire ſur la Décompoſition de l'Air atmoſphérique par le Plomb. Par M. Luzuriaga. Paris. 1784. 8°	The Author.

1785.	Presents.	Donors.
May 26.	Essays of Reform on the System of the Finances of Great-Britain. By J. Van Der Hey. Vol. I. London. 1785. 4°	The Author.
——	Reports of the Commissioners appointed to examine, take, and state the Public Accounts of the Kingdom. By John Lane. Vol. II. London. 1785. 4°	The Commissioners.
June 2.	Ionian Antiquities. London. 1769. fol.	Society of Dilettanti.
——	H. M. Marcard Beschreibung von Pyrmont. I. Band. Leipzig. 1784. 8°	The Author.
9.	Mathematical Tables. By Charles Hutton, LL.D. London. 1785. 8°	The Author.
——	Jacobi Dickson Fasciculus Plantarum cryptogamicarum Britanniæ. Londini. 1785. 4°	The Author.
16.	Reflexions on the Study of Nature: translated from the Latin of Linnæus. London. 1785. 8°	J. E. Smith, Esq.

AN

INDEX

TO THE

SEVENTY-FIFTH VOLUME

OF THE

PHILOSOPHICAL TRANSACTIONS.

A.

ACTINIA. See *Barbadoes.*

Air, Experiments on, by Henry Cavendish, Esq. p. 372. The diminution of the air by the electric ſpark depends upon the converſion of phlogiſticated air into nitrous acid, ibid. Deſcription of the apparatus made uſe of (Tab. XV.), ibid. Fig. 1, 2, and 3. explained, p. 373. Method of forcing the electric ſpark through the tube, p. 374. Experiments, when the electric ſpark was paſſed through common air, included between ſhort columns of a ſolution of litmus, ibid. When lime-water was uſed inſtead of the ſolution of litmus, ibid. With impure dephlogiſticated air, p. 375. Obſervations on the reſults of thoſe experiments, ibid. When air was confined by ſoap-lees, 375—377. Experiments to determine what degree of purity the air ſhould be of, in order to be diminiſhed the moſt readily, and to the greateſt degree, p. 376. Precipitation of a ſolution of ſilver by phlogiſticated nitre, p. 377. General reaſoning upon all the experiments, p. 378—380. Method of preparing the ſoap-lees, and dephlogiſticated air, uſed in theſe experiments, p. 380. Quantities of each uſed therein, p. 381. Experiment to determine whether all the phlogiſticated air of the atmoſphere is of the ſame nature, p. 381—382. See *Phlogiſticated Air.* Another cauſe of the

diminution of air by the electric ſpark, p. 382—384. No phlogiſton communicated by the electric ſpark, p. 384.

Air and Water, Experiments and obſervations relating to, by the rev. Joſeph Prieſtley, LL.D. p. 279. Opinions of Mr. Cavendiſh, Mr. Watt, and M. Lavoiſier, concerning decompoſed air, p. 280. The author's opinion of inflammable air, and fixed air, both which had been firſt advanced by Mr. Kirwan, ibid. Experiment to aſcertain whether any water is produced when the air is made to diſappear, p. 281. Other experiments of the ſame kind, p. 282. A peculiar circumſtance attending the melting of caſt iron with a burning lens, p. 283. Reſult of melting caſt iron in the bottom of a deep glaſs receiver, p. 284. Mr. Watt's concluſion thereon, ibid. A new and unexpected appearance which occurred in endeavouring to revive the calx of iron, or a calx ſaturated with pure air, ibid. Experiments to determine whether fixed air or water would be the produce of combining inflammable and dephlogiſticated air, p. 285—287. Obſervation thereon, p. 287. Reſults of other experiments with the calx of copper, and precipitate, *per ſe*, in inflammable air, p. 288. Reflection on the relation of water to inflammable air, and eſpecially Mr. Cavendiſh's ideas on the ſubject, ibid. The powerful attraction that charcoal or iron appear to have for water, when they are intenſely hot, p. 289. Experiment to aſcertain the influence of unperceived moiſture in the production of inflammable air, ibid. M. Lavoiſier's method of obtaining inflammable air with an hot iron tube and an hot copper tube, p. 290. Reſult of experiments made with charcoal, p. 291—296. And of others made with iron, p. 297. Obſervations thereon, p. 298. See *Balloons*. Experiment to aſcertain the quantity of inflammable air which may be obtained from any given quantity of iron, p. 298. The ſuppoſition that water conſiſts of two kinds of air endeavoured to be explained, p. 299. Proceſs of that inveſtigation, p. 300. Theory of that proceſs, p. 301. Analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itſelf imbibing dephlogiſticated air, p. 302. Experiments to prove that phlogiſton is a real ſubſtance capable of aſſuming the form of air by means of water and heat, p. 303. Caſt iron annealed remarkably different from that which has not undergone that operation, p. 304. Experiment with precipitate *per ſe*, ibid. Effects of heating iron in all the different kinds of air, p. 305. And of tranſmitting ſteam through a copper tube, to try the effects of ſpirit of wine, ibid. Charcoal of metals explained, p. 306. General inferences from the principal experiments, ibid.

Algol, See *Variable Stars*. Opinion of aſtronomers very unſettled concerning the change of its light, p. 134.

Anderſon, Mr. James. See *Mons Garou*.

Animal Flower. See *Barbadoes*.

Animals, preſerved in ſpirits, not ſo fit for anatomical examination, p. 340.

Architecture, &c. Sketches and deſcriptions of three ſimple inſtruments for drawing architecture and machinery in perſpective, by Mr. James Peacock, p. 366. Direc-

tions for placing the machines, &c. ibid. Deſcription of the drawing-board (fig. 1. tab. XIII.), ibid. Uſe of ditto, p. 367. How to draw all perpendicular lines at once, ibid. Deſcription of the compound-board (fig. 2. tab. XIV.), p. 368. Uſe of ditto, ibid. The T ſquare, its uſefulneſs, p. 369. Deſcription of the vertical-board (fig. 3. tab. XIV.), p. 368. Uſe of ditto, p. 370.

Artificial Spring, account of one, by Eraſmus Darwin, M. D. p. 1. Old diſuſed well near the doctor's houſe in Derbyſhire deſcribed, p. 2. St. Alkmund's well, its ſituation, ibid. A new ſpring diſcovered, ibid. Method of preventing the mixture of the old water with the new, p. 3. And of trying the height of the new ſpring, ibid. New water nearly of the ſame properties with St. Alkmund's well, p. 4. See *Mountains*. Increaſe in quantity and quality, ibid. And why, p. 6.

Aſa Fœtida, deſcription of a plant yielding it, in a letter from John Hope, M. D. p. 36. General deſcription, ibid—38. The aſa fœtida plant not ſatisfactorily known till Kæmpfer deſcribed it, p. 36. Letter from Dr. Pallas to Dr. Guthrie, with two of the plants, ibid. One of which planted in the Botanic Garden at Edinburgh, p. 39. The juice ſeems to be of the ſame nature with the officinal aſa fœtida, ibid.

Aſtronomical Obſervations, in two letters from M. Francis de Zach, Profeſſor of Mathematics, and Member of the Royal Academies at Marſeilles, Dijon, and Lyons, p. 137. Account of his obſervations of an eclipſe of the moon, made with Father le Fevre, Aſtronomer at Lyons, in the Obſervatory called *au grand College*, ibid. State of the ſky at the time of making the obſervation, ibid. Short teleſcopes preferable to long ones in obſerving eclipſes of the moon, p. 138. In eclipſes of the moon no greater exactneſs than that of a minute can be obtained, ibid. Agreement between the Imperial Aſtronomer, l'Abbé Hell's obſervations of the moon's ſpots made at Vienna, with thoſe made by M. Meſſier at Paris, ibid. Correſpondent altitudes of the ſun taken with a quadrant of three-foot radius, in order to adjuſt the pendulum-clock to apparent time, p. 139. Obſervations of the moon's eclipſe the 18th of March, 1783, p. 142. Father le Fevre's obſervations with a reflector fifty-five inches focal length, p. 143. Obſervations of Jupiter's ſatellites at Marſeilles, by M. Saint Jacques de Sylvabella, p. 144. Obſervation of the tranſit of Mercury, Nov. 12, 1782, at Marſeilles, by the ſame, p. 151. and by M. Wallot, at the Royal Obſervatory at Paris, ibid. Important remark upon the diameter of Mercury, p. 152.

Averrhoa Carambola, an account of the ſenſitive quality, in a letter from Robert Bruce, M. D. p. 356. Is a ſpecies of ſenſitive plant, ibid. Is differently affected by being touched in different manners, p. 356, 357. Method of confining the motion to a ſingle leaf, p. 357. Effect of impreſſing it by puncture, percuſſion, or compreſſion, ibid. Ditto on ſticking a pin into the univerſal petiolus, p. 358. On making a compreſſion with a pair of pincers on ditto, ibid. On the leaves being blown againſt each other, or the branches, ibid. Appearance of the leaves when ſhaded from any diſturbing cauſe in the day-time, p. 359. The leaves naturally perform a more extenſive

extenſive motion when going to ſleep than by external impreſſions in the day-time, ibid. Effects produced on a leaf by the burning lens, ibid. And the electrical ſhock, p. 360. Two other ſpecies of this genus mentioned by Linnæus, ibid.

B

Balloons, the cheapeſt method of filling them with the lighteſt inflammable air, p. 297.

Barbadoes, effects of the dreadful hurricane there in 1780, p. 334. The actinia, or a ſpecies of animal flower, then found in conſiderable numbers on that coaſt, p. 335.

Barker, rev. Robert, B. D. See *Stag's Head and Horns*.

—— Thomas, Eſq. See *Barometer*, &c.

Barometer, *Thermometer*, and *Rain*, Abſtract of a Regiſter of, at Lyndon, in Rutland, in 1784. By Thomas Barker, Eſq. Alſo of the Rain at South Lambeth, Surrey; and at Selbourn and Fyfield, Hampſhire, p. 481.

Baſe, account of the Meaſurement of a baſe on Hounſlow Heath, by Major-general William Roy, p. 385. Introduction, ibid.—390. The author's acknowledgements of the aſſiſtance of ſeveral ſcientific gentlemen, p. 389. 391. 417. 425. Particularly of the worthy Preſident of the Royal Society, p. 425. 458. Choice of the baſe, tab. XVI. p. 390. Firſt tracing of the baſe, and clearing of the ground, tab. XVI. p. 391. Soldiers preferred to country labourers, and why, ibid. A detachment encamped near Hanworth Summer-houſe, ibid. Clearing of the firſt ſection of the baſe, p. 392. Tracing of the ſecond and third ſections, p. 393. Half the ſoldiers quartered in the neighbouring villages to clear the third ſection, while the remainder ſmoothed the ſecond, ibid. Inſtruments made uſe of in the firſt and ſecond meaſurements, p. 394. Steel chain, tab. XVII. ibid. Conſtruction of the chain, ibid. Alteration of the chain, p. 395. Deal rods, tab. XVIII. p. 397. Different opinions concerning the beſt method of applying rods in meaſurement, p. 398. Meaſuring rods and ſtandard rod deſcribed, p. 399. Braſs ſtandard ſcale, and method of laying off the lengths of the deal rods, p. 401. Major-General Roy's ſcale compared with the Society's ſtandard, p. 402. Manner of aſcertaining the length of the deal rods with it, p. 403—405. Cheſts for holding the deal rods and ſtandards, p. 405. Stands for the meaſuring rods, tab. XVIII. and XIX. p. 406. Method of drawing a line through the air, parallel to the common ſurface from ſtation to ſtation, in equal diſtances of 200 yards or 600 feet, as in the figure at the top of tab. XVIII. p. 407. Nature of the moveable ſtands (towards the right-hand in tab. XVIII. and XIX.) explained, p. 408. Plans of the two tables (towards the right-hand in tab. XVIII.) explained, ibid. Plan of one of the ſquare tables (towards the left-hand of tab. XVIII.) with the ends upon the ſecond and third rods upon it in contact explained, p. 409. Mr. Smeaton's deal platforms, ſtanding on pickets driven into the ground, and properly levelled (tab. XIX.) deſcribed, p. 410. Boning teleſcope and rods, tab. XVIII. p. 411. Method of tracing the line of 200 yards, &c. from one fixed ſtand to the other, ibid. Boning teleſcope

telescope described, ibid. Boning rods, and their use, described, p. 412. Cup and tripod for preserving the point upon the ground, where the measurement was discontinued at night, and resumed next morning, p. 412. Difference between measuring with a rod of twenty feet, and with rods of twenty feet three inches, p. 412. Brass cup used on these occasions described, p. 413. 415. Nature of the tripod (tab. XVIII.) explained, p. 413. Method of suspending the plummet from any part of the deal rods indifferently, p. 414. Wheels for terminating, in a permanent manner, the extremities of the base, tab. XVIII. ibid. Mr. Mylne's improved machines for certainly referring to the same point on any occasion of correcting or repeating the work (represented in tab. XVIII.), p. 415. Rough measurement of the base with the chain, and determination of the relative heights of the stations by means of the telescopic spirit level (tab. XVI. and XVII.), p. 416. The first measurement of the south-east section of the base completed in three hours and a half, p. 417, 418. The same section re-measured, with the probable cause of the variation, p. 418. The operation with the chain suspended, and why, p. 419. Description of a hold-fast for the rear-end of the chain, invented by Col. Pringle (represented by two elevations adjoining in tab. XVII.), ibid. The operations resumed by measuring twice with the chain forwards and back again the second section of the base, ibid. The level of the first and second section of the base taken, with an account of the spirit-level made use of, p. 420. The first general table relating to the levels of the base explained, ibid. p. 421. Col. Calderwood's method of computing the difference between the hypothenusal distances of 600 feet each, and the reduced base distances, p. 420. Tracing of the base with the transit instrument, p. 421, 422. Consequence of delaying the use of that instrument, p. 422. Levels of the third section taken, and the rough measurement thereof with the chain completed, p. 422, 423. Result of the examination of the length of the chain in different temperatures, p. 423. Descent from the lower end of the base to the surface of the Thames at Hampton, p. 424. Measurement of the base with the deal rods, p. 425. Method of performing that operation, p. 426. Difference of measuring by coincidences and contacts accounted for, p. 427. The two first hypothenuses re-measured, with the result, p. 428. Their Majesties prevented by the weather from seeing the nature of the operations, p. 429. Method of discovering the error of the chain at each station, p. 429. Inconveniences occurring from the humid and dry state of the atmosphere, &c. p. 430. See *Expansibility*. Different comparisons of the rods with the standard, and the result, p. 432, 433. Near agreement between the result by the deal rods, and that furnished by the rough measurement with the chain, accounted for, p. 433. Measurement with the deal rods finished, ibid. Expansion of the deal rods, p. 434—436. See *Riga Red-wood*, *Deal Rods*. Mr. Ramsden's observations of the different expansions, p. 435. Operations on Hounslow-Heath totally suspended, p. 438. Comparison made in the beginning of September at Spring Grove, when the rods were in their dry or contracted state, and the remeasure-

ment

ment of the same space the next morning, when, being left out on purpose, they had contracted all the humidity they could, p. 439, 440. Description of the glass rods, ultimately used to determine the length of the base, tab. XIX. p. 441—449. The case containing the tube described, p. 441. Manner in which the middle of the tube is made fast to the middle of the case, p. 442. Fixed and moveable ends of the tube described, p. 443. Fixed and moveable apparatus described, p. 444—447. Mahogany lid on the top of the case described, p. 447. Method of ascertaining the length of the glass rods, p. 448. Disposition of the stands for the double measurement with the chain and glass rods; description of the apparatus then applied to the ends of the chain; and ultimate continuation of the measurement with the glass rods alone, tab. XVII. and XIX. p. 449. Reasons for rejecting the deal rods, ibid. Commencement of the operation for the double measurement, p. 450. 453. Apparatus attached to the first end of the chain, p. 451. Ditto for the last end (to the right-hand side of tab. XVII.), p. 452. Comparison of the contraction of steel and glass, with the error of the chain in defect, p. 454. Farther experiments with the chain given up, and why, p. 455. His Majesty honours the operation with his presence, p. 456. Equation of the deal rods and of the glass rods, on 3100 feet, ibid. Difference over-rated in the expansion of the 569 deal rods, p. 457. Measurement with the glass rods completed, p. 458. Equation of the 1370 deal rods and of the 1370 glass rods, with the difference over-rated in the total expansion of the deal rods, p. 459. Conjecture concerning the cause of the estimated and real expansion of the deal rods, ibid. p. 460. Description of the microscopic pyrometer, used for determining by experiment the expansion of the metals concerned in the measurement of the base, tab. XX. p. 461—472. The microscopic pyrometer (tab. XX.) described, p. 462—472. Use and description of the fixed microscope, p. 466. Ditto of the micrometer, p. 467. Use of two eye glasses in microscopes, p. 468. Scale of the pyrometer explained, p. 469. Account of the experiments with the pyrometer, p. 472—476. Ultimate determination of the base on Hounslow-Heath, p. 476—478. General table of the base, shewing the relative heights of the stations above the south-east extremity near Hampton Poor-house, the reduction of the hypothenuses, and the correction for the temperature of the glass rods, whereby the true length is obtained in the heat of 62 degrees, p. 479. Table of the expansion of metals, deduced from experiments made with the microscopic pyrometer in 1785, p. 480.

Bladh, Mr. See *Specific Gravities.*

Bouguer. See *Specific Gravities.*

Bradon, Leicestershire, observations on the lime-mountain there, p. 4.

Brisk, Mr. See *Vapour.*

Bruce, Robert, M. D. See *Averrhoa Carambola.*

Burning Mountains. See *Morne Garou, Caribbee Islands.*

C.

Calderwood, Col. See *Base*.

Caribbee Islands, reasons for supposing a communication between the burning mountains in them and the volcanos on the high mountains in America, p. 30. Shocks of earthquakes frequent near them, ibid.

Cavendish, Henry, Esq. See *Air*.

Combustion, Observations and Experiments on the Light of Bodies in a State of, by the Rev. Mr. Morgan, p. 190. Data to be attended to, ibid. Conclusion drawn from them, ibid. Manner in which the rays of light escape, when decomposed by heat, p. 191. Observations on the flame of a candle, p. 192. The best mode of shewing the escape of some rays by that degree of heat which will not separate others till increased, p. 193. Mr. Melvill's mode of examining bodies whilst on fire, ibid. Besides the increase or decrease of heat, there are other modes of retarding or accelerating the combustion of bodies, p. 194. Singularities in the colours of different flames accounted for, p. 195. Singular phænomenon attending a burning body explained, p. 196. Imperfection in Sir Isaac Newton's definition of flame, p. 197. Experiments on electric light, p. 198—206. General deductions useful in procuring any degree of certainty in any hypothesis, p. 198. Observations on phosphoric light, p. 208. Mr. Wilson's theory of the slow combustion of shells, ibid. Objections to that theory, p. 209—211. Postscript by Dr. Price, p. 211.

Construction of the Heavens, on the, by William Herschel, Esq. p. 213. Two opposite extremes to be avoided, if we hope to make any progress in an investigation of this delicate nature, ibid. Theoretical view of the universe, p. 214. Formation of nebulæ, ibid. Objections considered, p. 216. Optical appearances considered, p. 217—219. Result of observations, p. 219. The theoretical view of the universe, p. 224. Shewn to be perfectly consistent with facts, p. 226. I. Table of star-gages, p. 231—240. Problem, the stars being supposed to be nearly scattered, and their number, in a field of view of a known angular diameter being given, to determine the length of the visual ray, p. 241. The same otherwise, p. 243. We inhabit the planet of a star belonging to a compound nebula of the third form, p. 244. Difference between a crowd and a cluster of stars, p. 246. Use of the gages, p. 250. Table II. p. 252. Section of our sidereal system, p. 253. The origin of nebulous strata, p. 254. An opening in the heavens described, p. 256. Phænomena at the poles of our nebula, p. 257. Enumeration of very compound nebulæ or milky ways, p. 258. Some very remarkable nebulæ pointed out, ibid. Several extended nebulæ described, p. 260—262. A perforated nebula or ring of stars, p. 263. Planetary nebulæ, p. 263—266.

D.

D'Alembert. See *Rotatory Motion.*

Darwin, Dr. Erasmus. See *Artificial Spring.*

Deal Rods, table of the expansion of, with observations, p. 437. Are improper for very accurate measurement, p. 438.

De la Lande. See *Wires.*

De Luc, Mr. See *Specific Gravities.*

Double Stars. Catalogue of, by William Herschel, Esq. p. 40. Method of pointing out those stars in this collection which are not in Mr. Flamstead's Catalogue, p. 41. and of applying it, ibid. Precaution to be observed where other stars are very near those to be pointed out, p. 42. The observer should be furnished with Flamstead's Cœlestial Atlas, which must have the stars marked from the author's Catalogue, p. 43. Precaution in relation to examining the closest of the double stars, ibid. All the observations in this Catalogue are supposed to be made with a power of 460, unless marked otherwise, p. 45. Method of taking the measures of the distances, ibid. See *Micrometer.* Catalogue of double stars, p. 47. First class, ibid.—p. 65. Second class, p. 65—78. Third class, p. 78—90. Fourth class, p. 91—105. Fifth class, p. 105—118. Sixth class, p. 118—126. Additional errata to the Catalogue of Double Stars in vol. LXXII. p. 126. See *Variable Star.*

E.

Eagle-stone. See *Router.*

Earthquakes. See *Caribbee Islands.*

Eclipses. See *Astronomical Observations.*

Equatorial Micrometer, Mr. Smeaton's recommended, p. 348.

Euler, M. Leonhard. See *Rotatory Motion.*

—— M. John Albert. See *Ditto.*

Expansibility, observation on that of glass, with Mr. Cumming's pyrometer, p. 431. See *Deal Rods.*

F

Ferguson. See *Friction.*

Fixed Air and *Water* appear to consist of the same ingredients, p. 296. See *Air.*

Fordyce, George, M. D. See *Weight.*

Friction, on the Motion of Bodies affected by, by the Rev. Samuel Vince, A. M. of Cambridge, p. 165. This branch of natural philosophy, notwithstanding its importance

portance to the practical mechanic, has been, of all others, the moſt neglected, ibid. Muſſchenbroeck's, Helſham's, and Ferguſon's opinions of the law by which the motions of bodies are retarded by friction, p. 166. Euler's theory not true, ibid. Experiments to determine different properties of friction, ibid. p. 167. The friction of hard bodies in motion is a uniformly retarding force, p. 169. Experiments to determine whether friction, *cæteris paribus*, varies in proportion to the weight or preſſure, p. 170—172. Obſervations thereon, ibid. Experiments proving that the friction of a body does not continue the ſame when it has different ſurfaces applied to the plane on which it moves, but that the ſmalleſt ſurface will have the leaſt friction, p. 170—175. Reſult of the author's examination into the nature of the experiments which have been made by others, p. 176. Theory eſtabliſhed upon the principles already deduced, p. 178. Prop. I. to find the time of deſcent and the number of revolutions of a cylinder, or that circular ſection of a body on which it rolls down an inclined plane in conſequence of its friction, ibid. Cor. 1. p. 179. Cor. 2. ibid. Cor. 3. ibid. Cor. 4. p. 180. Cor. 5. ibid. Prop. II. the body being projected on an horizontal plane, with a given velocity, to determine the ſpace through which the body will move before it ſtops, or before its motion becomes uniform, p. 181. Caſe I. ibid. Caſe II. p. 182. Caſe III. p. 184. Definition of the centre of friction, p. 186. Prop. III. to find the centre of friction, ibid. Prop. IV. given the velocity with which a body begins to revolve about the centre of its baſe, to determine the number of revolutions which the body will make before all its motion be deſtroyed, p. 187. Prop. V. to find the nature of a curve deſcribed by any point of a body affected by friction, when it deſcends down any declined plane, p. 188.

G.

Goodricke, John, Eſq. See *Variable Star*.

H.

Hales, Dr. miſtake of his, p. 271.

Heat, diminiſhes the attractions of coheſion, chemiſtry, magnetiſm, and electricity, p. 364.

Hell, Abbé. See *Aſtronomical Obſervations*.

Helſham. See *Friction*.

Herſchel, William, Eſq. See *Double Stars*, *Conſtruction of the Heavens*.

Home, Mr. Everard. See *Marine Animal*.

Hope, John, M.D. See *Aſa Fœtida*.

Hunter, John, Eſq. See *Marine Animal*.

I.

Ithip, in Lancashire, manner of digging for water there, p. 6.

K.

Kirwan, Richard, Esq. See *Specific Gravities*.

L.

Landen, Mr. John. See *Rotatory Motion*.

Larbell river, has not the property of incrustation, p. 354.

Leeuwifer, M. See *Air and Water*.

Leach, has no anus, p. 341.

Le Veere. See *Astronomical Observations*.

Lightfoot, Rev. John. See *Motacilla*.

M.

Machinery in Perspective. See *Architecture*.

Marine Animal, Description of a new one, in a Letter from Mr. Everard Home, Surgeon, to John Hunter, Esq.; with a Postscript by Mr. Hunter, containing anatomical Remarks upon the same, p. 333. Reasons for believing this animal to be a non-descript, ibid. Where and when found, p. 334. See *Barbadoes*. By whom first observed, p. 335. Description of, ibid.—339. Postscript, by John Hunter, Esq. p. 340—343. See *Animals*. Anatomical division of the animal, p. 340. Its stomach and intestine described, p. 341. Differs materially from most animals with tentacula, ibid. See *Leach*, *Polypi*, *Univalve Fish*. Manner of voiding its excrements, p. 342. Enquiry into the manner of increasing its shell, ibid. Where most probable to be found, p. 343. Explanation of the figures, tab. XI. fig. 1. p. 344. Fig. 2. ibid.

Melvill, Mr. See *Combustion*.

Messier, M. See *Astronomical Observations*.

Meteors, are probably owing to an accumulation of electricity, p. 278.

Micrometer, improvement in Messf. Nairne and Blunt's, p. 46. See *Base*.

Microscope. See *Base*.

Morgan, Rev. Mr. See *Combustion*.

——— Mr. William. See *Hamilton*.

Morne Garou, a Mountain in the Island of St. Vincent, Account of, with a Description of the Volcano on its summit, in a Letter from Mr. James Anderson, Surgeon, p. 161. Difficulties in exploring the interior parts of the mountain, ibid. Which has

alſo been mentioned to have had volcanic eruptions from it, ibid. Of which, on examination, ſeveral indications appeared, p. 17. The author's friendly reception at Mr. Malloune's (where he lay) about a mile from the mountain, ibid. Arrives at the bottom of the mountain, attended by two negroes of Mr. Malloune's and a boy, p. 18. After climbing a rock, forty feet high, they come to ſome clear ground, belonging to a Mr. Gaſco, ibid. Who receives them kindly, p. 19. See *Weſt Indies*. Return to Mr. Gaſco's, being unable to get forward, p. 20. Deſcription and ſituation of his hut, ibid. Difficulties Mr. Anderſon encountered in his ſecond attempt, p. 21. Arrives at the bottom of a very high precipice, ibid. His dangerous ſituation on gaining the top, p. 22. Meets four negroes from Mr. Malloune's, with proviſions, p. 23. Diſcovers a ravin by which he might have gained the ſummit of the mountain with little difficulty, ibid. After a moſt diſagreeable night on the mountain, gets a ſight of the ſummit, p. 25. With a deſcription of it, ibid. Two negroes leaving him, and the reſt refuſing to proceed, he is forced to return, p. 25, 26. Sets out again, accompanied by Mr. Fraſer, and attended by two negroes, p. 26. Finds ſome beautiful plants and moſs (of which he ſaw none elſe in the Weſt Indies) in great abundance, ibid. Gains the ſummit, with a deſcription of the awful appearance of the excavation there, p. 27. 29. Burning mountain at the bottom thereof deſcribed, p. 27. Various minerals found on all parts of the mountain, p. 28. Author's reaſons for ſuppoſing it has but lately begun to burn, ibid. Two chalybeate lakes on the ſides of the crater, and a conjecture on the manner of their being ſupplied, ibid. Singular motion of the clouds on the mountain, p. 29. See *Caribbee Iſlands*. References to the figure, p. 31.

Motacilla, Account of an Engliſh Bird of that Genus, ſuppoſed to be hitherto unnoticed by Britiſh Ornithologiſts, obſerved by the Rev. John Lightfoot, p. 8. Generic characters, ibid. Marks evincing it to be of the ſpecies of Motacilla, p. 10, 11. Named after the Linnæan manner, p. 11. Seep the only author who can be ſuſpected of noticing this bird, p. 12. Which may not improperly be denominated the *Reed Wren*, ibid. Its haunts, ibid. Its food and note, p. 13. Its neſt, ibid. Materials and ſituation of its neſt, ibid. Number and deſcription of its eggs, p. 14. Conjectured to be a migrating bird, ibid.

Mountains, theory of the formation of, p. 4—7.

Myhe, Mr. See *Baſs*.

N.

New-England white-wood. See *Riga red-wood*.

Newton, Sir Iſaac. See *Combuſtion*.

Nitre, when much phlogiſticated, is capable of making a precipitate with a ſolution of ſilver, p. 378.

P.

Peacock, Mr. James. See *Architecture*, &c.
Phlogiſticated Air, whether many different ſubſtances are not in reality confounded together under this name, p. 381.
Phlogiſton. See *Air*.
Phoſphoric Light. See *Combuſtion*.
Pigott, Edward, Eſq. See *Variable Stars*.
Polypus, has no anus, p. 341.
Price, Dr. See *Combuſtion*.
Prieſtley, Rev. Joſeph. See *Air*, and *Water*.
Pringle, Col. See *Baſe*.
Pyrometer. See *Baſe*.

R.

Rain. See *Barometer*, &c.
Ramſden, Mr. his curious beam compaſſes, p. 402. See *Baſe*. His eaſy and ſimple way of obtaining the ſcale of his pyrometer, p. 471.
Reed-wren. See *Motacilla*.
Richmond, in Surrey, method of boring for water there, p. 6.
Riga Red-wood, more ſuſceptible of the effects of moiſture than New-England white-wood, p. 435.
Rotatory Motion. Of the Rotatory Motion of a Body of any Form whatever, revolving, without Reſtraint, about any Axis paſſing through its Center of Gravity, by Mr. John Landen, p. 311. When the axis, about which a body is made to revolve, is not a permanent one, the centrifugal force of its particles will diſturb its rotatory motion, &c. p. 312. To determine in what track, and at what rate, the poles of ſuch momentary axis will be varied, not an unintereſting propoſition, ibid. The ſolutions of that problem by M. Leonhard Euler, M. D'Alembert, and M. John Albert Euler, rectified by the author, ibid. Difference between him and the above gentlemen concerning the angular velocity and the momentum of rotation, p. 313. How to find a parallelopipedon that being by ſome force or forces made to revolve about an axis, with a certain angular velocity, ſhall move exactly in the ſame manner as any other given body, if made to revolve with the ſame force, about an axis paſſing through its centre of gravity, p. 315. Tab. X. fig. 1. explained, ibid. Method of finding how a parallelopipedon will revolve about ſucceſſive momentary axes paſſing through its center of gravity, p. 318. Fig. 2. and 3. explained, p. 318. 322. Fig. 4. ditto, p. 321. Fig. 5. ditto, ibid. Errors of M. Euler pointed out, p. 327, 328. To which M. D'Alembert's ſeem nearly ſimilar, p. 328.

Routtr, or Eagle-ſtone mountain in the Peak, its volcanic origin, p. 5.
Roy, Major General William. See *Baſe*.

S.

Schoenus Lithoſpermos, a ſpecies of graſs in the Weſt Indies, p. 19.
Senſitive Plant. See *Averrhoa Carambola*.
Sepp. See *Motacilla*.
Shells. See *Combuſtion*.
Smeaton, Mr. See *Equatorial Micrometer*, *Baſe*.
Specific Gravities, Remarks on, taken at different Degrees of Heat, and an eaſy Method of reducing them to a common Standard, by Richard Kirwan, Eſq. p. 267. Uſefulneſs of a comparative view of the weights of equal volumes of water, &c. ibid. A capital advantage derivable from a table of ſpecific gravities, ibid. Inſtrument for making experiments deſcribed, p. 268. Table of ſpecific gravities, p. 269. Remarks and obſervations by Bouguer, Mr. Bladh, Mr. De Luc, &c. p. 270. Advantages of this method pointed out, p. 271.
Stag's Head and Horns, Account of, found at Alport, in the Pariſh of Youlgreave, in the County of Derby. In a Letter from the Rev. Robert Barker, B. D. p. 353. Place and manner of diſcovering them, ibid. Not ſuppoſed to belong to a very old animal, p. 354. Are of the throſtel-neſt ſpecies, ibid. See *Throſtel-neſt Horns*. Conjecture how they were depoſited where found, ibid. See *Larkell*. Dimenſions of the horns found at Alport, p. 355. Ditto of a large pair of throſtel-neſts horns, ibid. Ditto of the horns of a ſeven-years old ſtag, ibid.
Summation of Infinite Series, Supplement to the Third Part of the Paper on, in 1782, by the Rev. S. Vince, M. A. p. 32.
Sylvabelle, M. Saint Jacques. See *Aſtronomical Obſervations*.

T.

Teleſcopes. See *Aſtronomical Obſervations*, *Wires*.
Tentacula, animals with tentacula conſiſt of an almoſt infinite variety, p. 343.
Terebella, a double-coned one, found by Mr. Cordiner, at Bamff, in Scotland, p. 343.
Thermometer. See *Barometer*, &c.
Throſtle-neſt Horns, deſcribed, p. 354.
Tuft, a kind of ſtone, deſcribed, p. 353.

U.

Vacuum, Electrical Experiments made in order to aſcertain the non-conducting Power of a perfect one, &c. by Mr. William Morgan, p. 272. Concluſion drawn from Mr. Walſh's

Walſh's experiments in the double barometer tube, not univerſally admitted, ibid. Account of the author's experiments to determine its truth or fallacy, ibid. Tab. IX. fig. 1. explained, ibid. Account of various electrical experiments, p. 274. 276. Mr. Brook's method of making mercurial gages, p. 276, 277. See *Meteors*.

Variable Star, Obſervations on a new one, in a Letter from Edward Pigott, Eſq. p. 127. Uſe of verifying ſtars ſuppoſed to be variable, ibid. Variation of the ſtar η Antinoi very ſimilar to that of Algol, though not exactly the ſame in any particular, ibid. Changes undergone by η Antinoi, p. 128. Order of the ſtars to which it was compared, ibid. Greater difference between the brightneſs of many ſtars in twilight and moon-light, or the leaſt hazy air, than in a dark and clear ſky, ibid. Journal of obſervations of η Antinoi, p. 129. Natural method of obtaining a point of compariſon for ſettling its periodical changes, p. 131. Computations (the rough length of a ſingle period being found) to obtain greater exactneſs, p. 132. Method of determining the time when η Antinoi was between its leaſt and greateſt brightneſs, ibid. Obſervations on its decreaſing brightneſs in 1783 compared with correſponding ones made in 1784, p. 134. Reſults of thoſe compariſons, ibid. Account of ſeveral attempts to determine the relative brightneſs of ſtars, p. 135, 136.

—— Obſervations on a new one, by John Goodricke, Eſq. p. 153. The ſtar β Lyræ diſcovered to be variable, ibid. Eight points of its variation in twelve days and nineteen hours, p. 154. Its relative brightneſs at its obſcuration in the third and ſeventh points, ibid. p. 161. Magnitudes of the ſtars to which it was compared during the progreſs of its variation, p. 155. Series of obſervations of its brightneſs and magnitude, ibid. Obſervations thereon, p. 161. Calculation of the times of the middle of its obſcuration in the third point, p. 162. Remarks thereon, ibid. Mr. Herſchel reckons β or γ Lyræ amongſt thoſe ſtars which he ſuppoſes to have undergone an alteration, p. 163.

Vince, Rev. Samuel, M. A. See *Summation of Infinite Series*, *Friction*.

Univalve Fiſh, their internal formation, p. 342.

W.

Wallot, M. See *Aſtronomical Obſervations*.

Walſh, Mr. See *Vacuum*.

Watt, Mr. See *Air and Water*.

Weight, An Account of ſome Experiments on the Loſs of, in Bodies on being melted or heated. By George Fordyce, M. D. p. 361. Several experiments made by the Doctor, which not being contradictory, are not worth laying before the Society, ibid. Account of a concluſive one in determining the loſs of weight in ice when thawed, and on being heated, ibid. p. 363. Conjectures of the cauſe of the acquiſition of weight on water's being converted into ice, p. 363. Method to determine which is true, ibid. Experiment to aſcertain whether there is matter abſolutely light, or which repels

repels instead of attracting other matter, p. 364. See *Heat*. Conjecture on the increase of the weight of ice on being cooled, ibid.

West Indies, difficulty of going through the woods there, p. 191.

Wilson, Mr. See *Combustion*.

Wires, A Description of a new System of, in the Focus of a Telescope, for observing the comparative Right Ascensions and Declinations of celestial Objects; together with a Method of investigating the same, when observed by the Rhombus, though it happen not to be in an equatorial Position, by the Rev. Francis Wollaston, LL.B. p. 346. The author twice disappointed in attempting to ascertain the right ascensions and declinations of the stars he had laid down, p. 346, 347. The rhombus is very good in theory, but seldom true enough to be depended on, p. 347. A very small error in observing the passage of a star makes a very material difference in the result, ibid. Trial with a square placed angularly, which seems to answer better, ibid. See *Equatorial Micrometer*. Tab. XII. fig. 1. explained, p. 347. Properties and advantages of the square preferably to the rhombus, p. 348. Method of rectifying the erroneous position of the instrument by the formula given by M. De la Lande in his Astronomy, p. 349. Formula constructed by the author's son, for investigating the comparative right ascensions and declinations of stars, ibid. Application of the formulæ, p. 351. How to deduce the angle of deviation from the true equatorial position in the new system of wires, p. 352.

Wollaston, Rev. Francis, LL.B. See *Wires*.

Z.

Zach, Francis de. See *Astronomical Observations*.

W.

FROM THE PRESS OF J. NICHOLS.

ERRATA.

VOL. LXXIII. PART I.

Page. Line.

28. 19. *for* was as 3 to 4 *read* was as 2 to 3.

VOL. LXXV. PART I.

283. 30. *for* amazing *read* amusing

285. 22. *for* on *read* in

296. 14. *for* in *read* on

301. 27. *for* air *read* airs

309. 23. *for* seems *read* seem

VOL. LXXV. PART II.

410. 4. *for* parallelopid *read* parallelopiped.